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ART. XVIII.— *The Selective Absorption of Solar Energy;* by S. P. LANGLEY.

INTRODUCTION.

IN 1800, Sir William Herschel published his remarkable investigations, in the Philosophical Transactions, on obscure heat, in which he reached the conclusion that heat is an entity distinct from light.

This view was modified by subsequent writers into the statement that each ray has three qualities—heat, light and chemical action, while at the present time many physicists have reached the further conception that heat, light and chemical action are not so much qualities inherent in any ray, as modes of the manifestation of one common energy; or, to state this view in the broadest manner, that the same ethereal wave will give us either heat, light or chemical action, according to the absorbent nature of the substance which receives it.

These last opinions, however, cannot be said even yet to be universally accepted by physicists.

Dr. J. W. Draper long since pointed out that the maximum of heat did not necessarily lie in every case below the red (though it does so in the prismatic spectrum), and that in a normal spectrum it would be in the orange. These conclusions as to the normal spectrum were unverifiable by direct experiment by any means he possessed. He found it impossible, with the most delicate thermopiles at his command, to get

sensible heat from the grating spectrum without gathering all that lay in the two halves of it together, and could consequently only infer the result of complete measures. But it followed that if it ever became possible to measure amounts of heat so minute, these conclusions could be verified on separate rays of the diffraction spectrum.

No one, so far as I know, has hitherto succeeded in measuring the heat from a diffraction grating except in the gross; by thus concentrating, for instance, the whole upper half and the whole lower half of its spectrum upon the pile, and so reaching some results not without value, even as thus obtained, but of quite other interest than those which may be expected when we become able to measure with close approximation the separate energy of each wave-length. Having devoted many years to the study of the solar radiant heat, by means of the thermopile, I was led to hope that by my long apprenticeship to the precautions needed with this instrument and the possession of the most delicate apparatus attainable, I might succeed better than my predecessors. I found, however, that though I got results, they were too obscure to be of any great value, and that science possessed no instrument which could deal successfully with quantities of radiant heat so minute, for the average heat in the diffraction spectrum does not under the most favorable circumstances reach one-tenth that in the prismatic one, and is usually much less even than this.

Impelled by the pressure of this actual necessity, I therefore tried to invent something more sensitive than the thermopile, which should be at the same time equally accurate—which should, I mean, be essentially a "*meter*" and not a mere *indicator* of the presence of feeble radiation, and was led by nearly a year's continual experimenting to the construction of the Bolometer (*βολῆ μετρον*), an instrument, the details of whose construction are described in the Proceedings of the American Academy of Arts and Sciences, vol. xvi (1881). With this apparatus, the experiments on the diffraction spectrum were resumed: the first entirely unquestionable evidence of measurable heat, in a width so small as to be properly described as linear, having been obtained on October 7, 1880. Nearly the whole year 1880 passed in modifications of the instrument, or in the making of these measures which gave promise from the first of bringing results of value.

It will be seen that they afford almost all the experimental evidence the subject admits of, that every ray, whether lying in the "chemical," "visible" or "heat" region, is capable of making itself known as heat; and that the maximum of heat in the normal spectrum is near the yellow.

Further, by taking all the observations twice daily, at times

when the atmospheric absorption is very different, we are able to calculate (for the first time), the amount of this absorption for each separate spectral ray. These researches are necessarily long and difficult, but they have led to some very unexpected results. The reader who wishes to pass at once to these results will find them in the Summary at the close of this memoir.

Preliminary Observations.

The measurements with the grating possess the inestimable advantage of enabling us to fix the wave-length of every ray measured; but, while the heat in the grating spectrum is, as has just been said, at best, less than one-tenth that in the prismatic, the latter is itself, when taken in portions so narrow as to be approximately homogeneous, almost insensible. The difficulties of measuring heat with the grating are greatly complicated by the overlapping of the spectra. In these first measures, which were carried to a wave-length of one one-thousandth of a millimeter,* I have employed two of the admirable gratings of Mr. Rutherfurd, one containing 17,296 lines to the inch, or 681 to the millimeter; and one, half that number, both ruled upon speculum metal.

I have used a slit at a distance of 5 meters, without any collimator, and have kept the grating normal to the optical axis. It will be seen then that the rays have passed through no absorbing medium whatsoever, except the sun's atmosphere and our own.

The rays from the grating fall upon a concave speculum (whose principal focal distance is about 1 meter), and from this are concentrated upon the mouth of the bolometer, forming a narrow spectrum, which passes down the case of the instrument and falls upon the bolometer thread. As this thread moves along the spectrum parallel to the Frauenhofer lines its coincidence with one of them is notified by a lowering of its temperature and a deflection of the galvanometer. The instrument is of course equally sensitive to the invisible radiation as to the visible. It is important to observe that no screen is interposed between the bolometer and the grating, for the temperature of the screen itself, as it is replaced or withdrawn, will certainly affect such measurements as these. Through the whole course of the experiment the bolometer is uninterruptedly exposed to radiations from the grating, whether reflected by it or emanating from its own substance. The interruption of the solar radiation is effected at the other end of the

* Through these measures the unit of wave-length will be the *micron* (μ)= $\frac{1}{1000}$ millimeter, or 10,000 times the unit of Ångström. Thus the wave-length of Frauenhofer's "A" is here written $0^{\mu}{\cdot}76$.

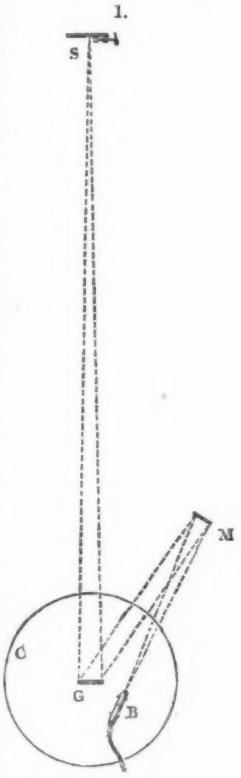
train, 5 meters beyond the grating itself. In the gratings employed one of the second spectra is very feeble, or almost lacking. The rays of the second spectrum are necessarily superposed on those of double the wave-length in the first; and as all evidence of solar radiation in the most sensitive apparatus at the sea-level dies out near $\lambda = 0^{\mu}{\cdot}3$ in the ultra violet, it follows that we can measure down in the first spectrum as far as $\lambda = 0^{\mu}{\cdot}6$, or in fact further, without any fear whatever of our results being affected by the underlying second spectrum, even if that were a strong one. Underlying $0^{\mu}{\cdot}7$, which is near the limit of the red in the first spectrum, is $0^{\mu}{\cdot}35$ in the second, where heat is practically still negligable. We have, therefore, knowing the amount of heat in the second spectrum at $0^{\mu}{\cdot}5$, and knowing that our ultimate point of measurement at $1^{\mu}{\cdot}0$ in the first spectrum overlies $0^{\mu}{\cdot}5$ in the second, the means of asserting with confidence that no considerable error can be introduced from this cause, after an allowance has been made here for the minute effect of this second spectrum. An allowance is also made to reduce the effect to that which would have been observed with a grating so coarsely ruled as to cause no considerable deviation from the slit of any portion of the spectrum measured. The bolometer (in a constant position relative to the concave mirror such that the optical axis of the latter bisected the angle between its central thread and the center of the grating), was moved, together with the mirror, by a tangent screw in arc, so that the spectrum appeared to traverse its face.

The actual angular deviation of any ray under examination was obtained from a divided circle on which the arm carrying both mirror and bolometer moved. A particular description is not given, as the whole apparatus was replaced by a more perfect one later. That actually used will be intelligible by the sketch, fig. 1, where S is the slit, G the grating, M the concave mirror, B the bolometer, and C the divided circle.

The light came from the silvered mirror of a heliostat, passing through the slit at a distance of about 5 meters from the grating, which was bolted immovably above the center of the circle of a massive dividing engine, with the grating's plane always perpendicular to the line joining its center and the slit. The mirror and the bolometer, with their attachments, were fastened to this movable circle.

An allowance has been made for the absorption of speculum metal and silver, but the absorption of the iron strips of the bolometer has only been indirectly allowed for. This has been done by comparison with the action of a bolometer with lamp-

blacked surface. It will be seen hereafter that the whole experiment has been repeated with a lampblacked bolometer without in any way affecting the results. The wave-lengths are derived from the measured angles by the use of the formula $ns\lambda$ = sine i + sine r, where n is the order of the spectrum, s the space between the lines of the grating, λ = the wave-length of the radiation, i the angle of incidence (in the present instance 0°), and r the angle of diffraction.



In the early observations it appeared from the examination of the diffraction spectrum up to $\lambda = 1^{\mu}{\cdot}0$ that the energy in the invisible part as far as this, was much less than in the visible. Nothing definite is even at this time known to physicists as to the extent of the normal solar spectrum; but the prismatic spectrum is still very commonly supposed to be limited by theoretical considerations to an extent little greater than this; and one of those most conversant with the subject has treated this wave-length (i. e. $1^{\mu}{\cdot}0$) as marking the limit of everything known to exist.*

It seemed at first then improbable that the heat below the red should materially exceed or even equal that above it; for this would demand (since the heat shown by the last ordinate at $\lambda = 1^{\mu}{\cdot}0$ is very small) an extension of the curve of heat, as obtained from the grating, to a distance enormously beyond the furthest

* Draper, "On the Phosphorograph of a Solar Spectrum, and on the Lines in the Infra-red Region." Proceedings of the American Academy, vol. xvi, p. 233, Dec. 1880. He asks, "Do we not encounter the objection that this wave-length $10{\cdot}750^{m-10}$ (the limit of Captain Abney's map) is altogether beyond the theoretical limit of the prismatic spectrum?" Previous measurements of heat had, it will be remembered, been made by comparing its total amounts, in the visible and invisible prismatic spectrum, which gives us no knowledge as to wave-lengths in any case, and wave-lengths in the dark-heat region, had been estimated, by hazardous extra-polations from contradictory formulæ—formulæ which profess a theoretical basis, but contradict each other. Thus Müller finds by Redtenbacher's formula a wave-length of nearly $5^{\mu}{\cdot}0$ for the extreme solar heat rays, Draper (as we have just seen) a wave-length of but $1^{\mu}{\cdot}0$ for the same rays, etc. All these formulæ (Briot's, Cauchy's, etc.) agree well with the observations in the visible spectrum, which they have in fact been originally deduced from. They contradict each other thus grossly when used for extra-polating the place of the extreme infra-red rays, whose real place we give later from actual measures.

limit then assigned to the normal spectrum by experiment. The writer's further investigations, however, led him to believe that this immense and unverified extension really existed, and to thus confirm by independent means the statements of Tyndall and others, as to the great heat in this region. He was unable to determine its exact limit with the grating as then used, on account of the over-lapping spectra, but was, some two years since, led, from experiments not here detailed, to suspect the existence of solar heat at a distance of nearly four times the wave-length of the lowest visible line, $A(\lambda=0^{\mu}\cdot76)$ or at $\lambda=3^{\mu}\cdot0$.

We receive all the solar radiations through an absorbing atmosphere, and it is of the first consequence to determine the rate of this selective absorption for each separate ray. This has (owing to the difficulties before alluded to) never yet been, so far as I know, attempted. It forms a prominent part of the present design.

The great difficulty in this investigation, after the provision of a sufficiently delicate heat-measurer, lies in the varying amount of radiant energy which our atmosphere transmits, even for equal air-masses. The solar radiation is itself sensibly constant, but the variations in the radiant heat actually transmitted are notable, even from one minute to another under an apparently clear sky. The bolometer, in fact, constantly sees (if I may use the expression) clouds which the eye does not. That these incessant variations are in fact due to extraneous causes and not to the instrument itself, has been abundantly demonstrated by measurements on a constant source of heat.

Those taken, for instance, on a petroleum lamp, so placed as to give nearly the same galvanometer deflection as the sun did, were found to indicate a probable error, for a single observation, of less than one per cent.

The variations from minute to minute (under a visually clear sky) amount frequently to ten times the probable instrumental error, and they can only be eliminated by repeating the observations a great number of times on many different days. Actually, twenty-nine such days' observations have been made (as appears below) in the preliminary series, but it would be an error to suppose that this number was obtained without the sacrifice of a still larger number on which the apparatus was prepared, and the day spent without results, owing to the still more considerable atmospheric changes between morning and afternoon. Even of the twenty-nine days cited, and which may be considered exceptionally fair, it will be seen that in only ten cases did the sky continue sufficiently constant in the morning and afternoon to allow complete series to be taken.

It will be understood that we aim to make at least two sets of measures throughout the spectrum daily, one when the rays

have been little absorbed (at noon), the other when they have been greatly absorbed (in the morning or afternoon). The mass of air through which the rays pass is taken proportional to secant, ζ for zenith distances less than 65°, and for those greater to $\frac{\cdot 0174 \text{ tabular refraction}}{\text{cos. apparent altitude}}$, and in both cases to the actual barometric pressure. It is expressed in units, each of which is represented by the pressure of one decimeter of mercury at the sea-level. As the barometric pressure there is $7{\cdot}6^{dm}$, $t^{7{\cdot}6}$ gives the transmission for an entire atmosphere. The coefficient of transmission then for one atmosphere ($t^{7{\cdot}6}$) is the proportion of the radiation transmitted by a sun in the zenith to an observer at the sea-level, and this is here shown to vary greatly for each ray. Thus by reference to table III, we find of three solar rays whose wave-lengths are ·375, ·600, ·1,000, that of the ray whose wave-length is $0^{\mu}{\cdot}375$ (in the ultra violet) 61 per cent of the original energy would be absorbed and 39 transmitted, of wave-length $0^{\mu}{\cdot}600$ (in the orange) 36 per cent would be absorbed and 64 transmitted, of wave-length $1^{\mu}{\cdot}000$ (in the infra-red) 20 per cent is absorbed and 80 transmitted, etc.

Allegheny Bolometer Observations on the Solar Diffraction Spectrum previous to Mt. Whitney Expedition.

The following list shows the dates at which bolometer observations were made at Allegheny up to June, 1881, for the measurement of heat in the spectrum and the determination of atmospheric transmission, by the comparison of noon and afternoon measures. Those days on which noon measurements were taken which were rendered useless for this purpose by subsequent changes in the condition of the sky or by other causes are indicated by an asterisk. It will be seen that of twenty-nine days of observation only ten could be fully utilized.

Dates: 1880, Nov. 12,* Dec. 11,* Dec. 18.* 1881, Jan. 12,* Jan. 18,* Jan. 28, Feb. 2, Feb. 3,* Feb. 5,* Feb. 17, Feb. 19,* Feb. 22,* Feb. 26,* Mar. 2,* Mar. 10.* Mar. 11,* Mar. 25,* Mar. 28,* Apr. 7,* Apr. 16,* Apr. 22, Apr. 23, Apr. 28,* Apr. 29, Apr. 30, May 4,* May 26,* May 27,* May 28.

The following table gives the observed galvanometer deflections reduced to a scale on which the readings are proportional to the current passing through the galvanometer.

d_i = galvanometer deflection with high sun
d_{ii} = " " " low sun.

TABLE I.

$\lambda =$		$0^{\mu}.375$	·400	·450	·500	·600	·700	·800	·900	1·000
Jan. 28, 1881	$d_{,}$	--	101	--	374	383	320	221	144	102
	$d_{,,}$	--	43	--	167	268	215	221	116	78
Feb. 2	$d_{,}$	34	80	215	289	307	293	175	--	93
	$d_{,,}$	3	20	61	104	141	195	91	--	47
Feb. 17	$d_{,}$	23	62	120	232	260	227	188	--	71
	$d_{,,}$	8	25	58	110	133	151	80	--	39
Apr. 22	$d_{,}$	19	43·5	154	236	262	239·5	177·5	123·5	98
	$d_{,,}$	6·5	17	63	119·5	171·5	180·5	122·5	89·5	84
Apr. 23, A. M.	$d_{,}$	--	59	152	206	263	227	191	121	94
	$d_{,,}$	--	41	124	189	258	257	187	122	96
Apr. 23, P. M.	$d_{,}$	--	59	152	206	263	277	191	121	94
	$d_{,,}$	--	32	103	124	188	198	140	80	66
Apr. 29, A. M.	$d_{,}$	13	29	113	151	235	235	139	100	89
	$d_{,,}$	10	22	65	126	156	197	135	93	86
Apr. 29, P. M.	$d_{,}$	13	29	113	151	235	235	139	100	89
	$d_{,,}$	5	8	49	62	107	116	72	58	66
Apr. 30	$d_{,}$	21	55	121	186	245	259	175	119	90
	$d_{,,}$	18	33	97	148	220	232	166	97	80
May 28	$d_{,}$	8	34	99	109	144	134	89	64	52
	$d_{,,}$	--	2	9	27	52	66	61	33	39

The next table gives the sun's position, and the corresponding air-mass for each series in the previous table.

In this table, β denotes the reading of the barometer and M is determined from the formula

$$M = \frac{\text{·0174} \times \text{tabular refraction}}{\text{cos. app. altitude.}}$$

TABLE II.

Date of Observation.	High Sun.				Low Sun.			
	Sun's Hour Angle.	Sun's Zenith Distance.	Barometer ($\beta_{,}$).	Air Mass ($M_{,}\beta_{,}$).	Sun's Hour Angle.	Sun's Zenith Distance.	Barometer ($\beta_{,,}$).	Air Mass ($M_{,,}\beta_{,,}$).
	h m	° ′	d m		h m	° ′	d m	
Jan. 28, 1881	0 00	58 29	7·45	14.25	2 57	71 28	7·45	23·24
Feb. 2	0 09	57 09	7·39	13·63	3 00	70 45	7·39	22·24
Feb. 17	0 38	52 57	7·43	12·33	2 56	66 09	7·42	18·25
Apr. 22	0 12	28 13	7·36	8·35	4 36	66 22	7·36	18·32
Apr. 23, A. M.	0 11	27 49	7·40	8·37	2 45	45 30	7·40	10·56
Apr. 23, P. M.	0 11	27 49	7·40	8·37	4 26	63 57	7·40	16·85
Apr. 29, A. M.	0 06	25 50	7·35	8·17	3 11	48 46	7·35	11·15
Apr. 29, P. M.	0 06	25 50	7·35	8·17	5 23	73 36	7·35	35·73
Apr. 30	0 04	25 31	7·41	8·21	3 54	56 31	7·41	13·43
May 28	0 11	19 03	7·32	7·75	5 33	71 14	7·32	22·33

By combining the high and low sun observations of each day separately, coefficients of atmospheric transmission are obtained by using the formula

$$\log t = \frac{\log d_{,,} - \log d_{,}}{M_{,,}\beta_{,,} - M_{,}\beta_{,}}$$

where t is the coefficient of vertical transmission by air at a barometric pressure of one decimeter. A tabular statement of these coefficients has been prepared, but the average or adopted value only is here given.

TABLE III.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
Adopted t	·884	·892	·909	·923	·942	·955	·965	·970	·971
$t^{1.6}$	·392	·420	·485	·544	·636	·705	·763	·794	·799

It is important to notice that (contrary to a generally received opinion), *the transmissibility of the atmosphere is here found to be greatest for the infra-red rays.*

All good noon observations have been reduced to a uniform battery current of 0·25 webers, and the results, arranged in two sets, the first for winter and the second for spring measures.

These tables are not here given but the average results are as follows:

TABLE IV.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
Winter d_r (mean of 7 series)	31	88	190	294	328	259	172	111	91
Spring d_r (mean of 9 series)	18	57	139	218	281	271	188	121	94

The average noon deflections for winter and spring, given in the previous table, require further correction; first, for the over-lapping portion of the (weak) second spectrum, which is considered from provisional experiments to have here an intensity one-thirtieth that of the first. Second, for the selective absorption by silver surfaces. Third, for the selective absorption by one surface of speculum metal. Fourth, for the diminution of heat in the diffraction spectrum with increase of the angle of diffraction, which is here provisionally taken as proportional to secant r. The selective absorption by the material of the bolometer is here treated as negligable.

These corrections are expressed as factors by which the uncorrected deflections are to be successively multiplied, except in the case of the first correction which is to be subtracted. (The second and third corrections have been determined here by special researches on metallic absorption, which will form the subject of a separate memoir).

The researches here on the selective absorption of lampblack, it should be added, are incomplete and the value given may be yet subject to a further correction due to this error.

TABLE V.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
Correct'n									
I (subtr.)	0	0	0	0	0	0	$\frac{1}{30} \times d_{.40}$	$\frac{1}{30} \times d_{.45}$	$\frac{1}{30} \times d_{.50}$
II (factor)	3·005	2·067	1·606	1·448	1·301	1·227	1·192	1·166	1·145
III "	2·000	1·923	1·802	1·695	1·550	1·460	1·408	1·389	1·370
IV "	1·034	1·039	1·051	1·064	1·096	1·138	1·193	1·266	1·366

We have been measuring thus far "heat" by which we mean the solar energy as interpreted by certain agents (that is, silver, lampblack, etc.), in our apparatus. In the degree in which we have above eliminated the selective absorption peculiar to each of these agents, are we entitled to speak of the resultant values, as proportional to the solar energy itself. We do not suppose ourselves to have accomplished so untried and difficult a task with exactness, but regard these curves as useful as a first approximation to the absolute energy curve.

These corrections being applied, the final values of noon deflections at Allegheny become

TABLE VI.*

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
$d_{,}$ (cor.) winter, 1881,	192·6	363·4	579·3	767·9	724·9	527·9	338·3	215·4	173·6
$d_{,,}$ " spring, 1881,	111·9	235·4	423·7	569·6	621·0	552·5	372·3	238·0	234·6

The mean air mass for winter = 13·88
" " " spring = 9·33

We now proceed to the calculation of the energy outside the atmosphere for homogeneous rays with the data which have been given. For this purpose we have used the formula

$$\text{Log } E = \log d_{,} - M_{,} \beta_{,} \log t.$$

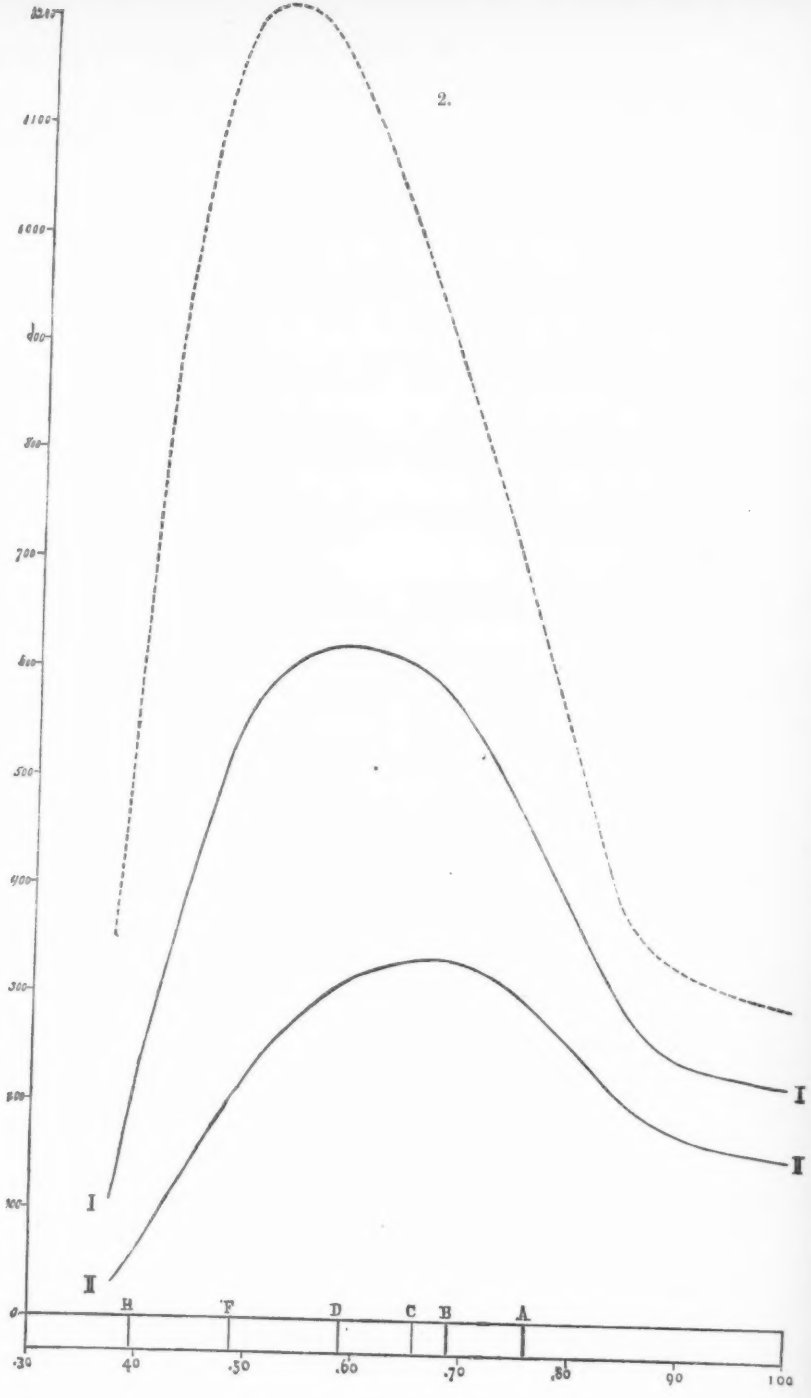
Where E is the energy in any ray outside the atmosphere (i. e. before telluric absorption), $d_{,}$ the average galvanometer deflection at noon for the same ray, $\beta_{,}$ the barometer pressure in units of one decimeter, or the mass of air in the vertical column; $M_{,} \beta_{,}$ the corresponding air-mass for the sun's zenith distance at noon, and t the adopted coefficient of transmission.

The following table has been prepared with the values observed in the spring of 1881, using mean coefficients of transmission, to show the relation between energy outside the atmosphere and that for high and low sun at Allegheny, the various actual absorbing air-masses at the low sun observations being reduced to a uniform value, double that at high sun.

TABLE VII.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
E = energy before absorption,	353	683	1031	1203	1083	849	519	316	309
$d_{,}$ = energy after absorption (corrected high sun),	112	235	424	570	621	553	372	238	235
$d_{,,}$ = energy after absorption (corrected low sun),	27	63	140	225	311	324	246	167	167

* It will be seen that although the winter absorbing air-mass was nearly half as large again as in the spring, the heat received from the shorter wave-lengths was actually greater in the winter. It appears probable, then, that the transmissibility of the atmosphere for the light-producing radiations is relatively greater in winter than in spring. As this effect may be connected in some way with the unequal prevalence of atmospheric moisture at the two seasons, it may be well to state that the tension of aqueous vapor during the winter observations was in the neighborhood of 2 millimeters, in the spring of 8 millimeters.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E can be computed from $d_{,}$ and $d_{,,}$ by the formulæ already given, and with these values the curves in fig. 2 have been plotted.

The middle curve (I) is that at high sun. Except for the heat below wave-length ($1^{\mu}\cdot0$), the area of the curve may be considered to represent the heat actually observed by the actinometers, at noon, as presently given.

The lower curve (II) is that at low sun. Its area is proportional to the heat received when the sun shone through double the absorbing air-mass that it did at noon.

The upper dotted curve is "the curve outside the atmosphere." Its area will give the heat, which would be observed if our apparatus were taken wholly above the absorbing air, and the distribution of this heat (energy) before absorption. Knowing the values in calories corresponding to the middle curve, we readily obtain the absolute heat before absorption (the Solar Constant).

It should be noticed that if we had attempted to deduce this latter value, by applying our logarithmic formulæ directly to ordinary actinometric observations (i. e. to observations where only the indiscriminate effect of all heat rays is noted by the thermometer) made at high and low sun, we should have obtained a quite different result. This has been the usual process, but it can never be a correct one; for these exponential formulæ are in theory only applicable to homogeneous rays, and the departure from theory here involves an error which is demonstrably large.

The above values (in table VII) are relative only. To obtain absolute ones we have now to combine this result with the actual measurements of solar radiation in calories, or other units furnished by actinometers under approximately the same conditions. We shall at the same time thus obtain a preliminary value for the Solar Constant. Taking the mean of our observations with the Violle and Crova actinometers on clearest days, we have 1·81 calories observed at Allegheny in March, 1881. This is the absolute amount of heat represented by the area of a completed "high sun" curve.

To this result, the energy distributed through the whole spectrum has contributed, while our bolometer measurements in the diffraction spectrum end at wave-length $1^{\mu}\cdot00$. Nevertheless, since we do in fact know from subsequent measures (to be given later) where the effective spectrum ends, we can by the aid of these later measures prolong the curves and obtain their relative areas with close approximation. In this way we determine, by measuring the charted areas, and making allowance for the (here) uncharted area below $\lambda = 1^{\mu}\cdot0$:

Area Outside Curve above $= 1^{\mu}\cdot000$	47·26
Area Outside Curve below $= 1^{\mu}\cdot000$	26·49
Total	73·66
Area High Sun Curve above $= 1^{\mu}\cdot000$	26·96
Area High Sun Curve below $= 1^{\mu}\cdot000$	20·00
Total	46·96

The ratio of these areas is $\frac{\text{Area Outside Curve}}{\text{Area High Sun Curve}} = \frac{73\cdot66}{46\cdot96} = 1\cdot569$

We have, then, adopting 1·81 cal. as the solar radiation at Allegheny with clear sky, 1·81 cal. × 1·57 = 2·84 calories as an approximate value of the Solar Constant.

In all these observations, the object has been to avoid the registering of small variations analogous to the Frauenhofer lines, and to give only the general distribution of the energy. The mapping of the interruptions of the energy caused by visible or invisible lines or bands forms a distinct research, and the results are given later in the present article.

We find from these preliminary observations, that the maximum energy in the normal spectrum of a high sun at the earth's surface is near the yellow, and that the position of the maximum of heat does not in fact differ widely from that of the maximum of light. It has been long known that certain ultra violet and violet rays were much absorbed, but it has been supposed that the absorption increased also in the infra-red, so that the luminous part of the spectrum was on the whole the most transmissible.

But we see here, not only how enormous the absorption at the violet end really is, but that the light rays have suffered a larger absorption before they reach us than the "heat" rays (i. e. than the extreme red and infra-red rays) a conclusion opposed to the present ordinary opinion, and if true, of far-reaching importance. For if this "dark" heat escapes by radiation through our atmosphere more easily than the luminous heat enters, our view of the heat-storing action of this atmosphere, and of the conditions of life on our planet must be changed. Within the limits of the present charts the "dark" heat apparently does so escape.

We can from the data now gathered as to the rate of absorption for each ray, compute the value of the heat or energy before absorption (the solar constant) by a new process which is in strict accordance with theory. This preliminary value indicates that the true solar constant is larger than that commonly given. The ratio of the dark to luminous heat has been so wholly changed by selective absorption that we must

greatly modify our usual estimates not only of the sun's heat radiation but of his effective temperature.

The sun to an eye without our atmosphere would appear of a bluish tint.

In spite of the care with which the experiments on which the above conclusions rest have been conducted, owing to the importance of the subject, and to their departure in some respects from received opinion, it seemed desirable to repeat them under conditions differing as much as possible from those in which they were made. If the preceding conclusions are sound, we ought to find like results, by actually ascending to a great height, and directly measuring there, as well as below, the absorptions which each ray has actually undergone.

Expedition to Mount Whitney.

In July, 1881, an expedition, fitted out, and instrumentally equipped at the Allegheny Observatory, proceeded in the writer's charge but with the aid of transportation furnished by the War Department and under the official direction of General W. B. Hazen, U. S. A., Chief Signal Officer of the Army, to Mt. Whitney in Southern California, where these observations and others were repeated at two contiguous stations at very different absolute altitudes. The results will shortly appear in an official publication (some of the drawings prepared for which have been used for the present memoir, by the kind permission of General Hazen). At present it is sufficient to say of them that the conclusions just rehearsed were confirmed and extended.

While on the mountain, at an elevation of 13,000 feet, a hitherto unrecognized extension was observed by the bolometer of the infra-red *prismatic* spectrum in the vicinity of the great absorption band, marked on our prismatic chart as Ω and beyond it, and on the return to Allegheny it was found that this last observation could still be continued in our lower atmosphere. The generosity of a citizen of Pittsburgh had enabled the observatory to provide for the expedition several pieces of special apparatus. Among these was a Foucault Siderostat, of the dimensions of that at the Paris Observatory, but of a much improved design; and a special apparatus (the spectro-bolometer), to enable the deviations of the invisible rays to be measured with an error of less than a minute of arc, etc., and this apparatus has been used in the new research we now describe. (The Siderostat was made by A. Hilger of London, the spectro-bolometer by W. Grunow, of New York, and both have been very satisfactory.)

Sir William Herschel, in 1800,[1] showed that heat extended below the visible spectrum. He found that about one-half the spectrum consisted of obscure and one-half of luminous heat. Seebeck and Melloni in various memoirs showed that the disposition of the heat depended on the substance of the prism, and that this was due in part to its absorption.

In 1840, Sir John Herschel[2] gave a thermograph of the invisible spectrum indicating unequal absorption below the red.

Dr. J. W. Draper, in 1842,[3] observed three wide bands in this region which he called α, β, γ. In 1846, Messrs. Foucault and Fizeau appear to have observed the same lines. Dr. Draper[4] states that prior researches lead him to believe that the hottest part of the normal spectrum will be found in the yellow.

Dr. J. Müller[5] gives a construction showing how we may, from the distorted prismatic spectrum, obtain the true or normal dispersion. Dr. Müller conjectures that the wave-length of the extremest infra-red ray is about $1^{\mu}{\cdot}8$, and from his diagram it appears that nearly two-thirds of the heat is below the visible portion.

Tyndall[6] gives the position of the maximum of heat in the prismatic spectrum and estimates the invisible radiation of the sun to be twice the visible.

In 1871, Lamansky[7] gave a drawing showing three gaps in the continuity of the infra-red curve as observed by the thermopile. Lamansky repeats the usual statement that these infra-red rays are strongly absorbed by the atmosphere.

In 1879, Mons. Mouton[8] in a valuable memoir, speaks of there being four known bands in the infra-red whose wave-lengths are $0^{\mu}{\cdot}85$, $0^{\mu}{\cdot}99$, $1^{\mu}{\cdot}23$, $1^{\mu}{\cdot}48$, and gives $1^{\mu}{\cdot}77$ and $2^{\mu}{\cdot}14$ as wave-lengths he supposes himself to have identified.

If our charts be correct there is no considerable band at $1^{\mu}{\cdot}48$, and $2^{\mu}{\cdot}14$ which he marks as the termination of the spectrum is in fact the hottest point in its neighborhood.

It seems probable, however, that he had perceived by his ingenious method the existence of the band whose wave-length on our charts is marked $1^{\mu}{\cdot}37$, and in doing so had reached the furthest band then certainly observed.

Captain Abney,[10] in 1880, mapped by photography the infra-red prismatic spectrum as far as wave-length $1^{\mu}{\cdot}075$ with a precision and completeness till then wholly unknown, besides

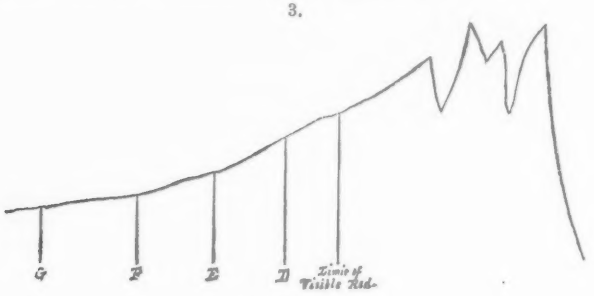
[1] Phil. Trans., 1800. [2] Phil. Trans., 1840. [3] Phil. Mag., May, 1843.
[4] Phil. Mag., 1857. [5] Poggendorf's Annalen, vol. cv. [6] Phil. Trans., 1866.
[7] Monatsbericht Konig. Acad. Wissenschaft., Berlin, 1871. Phil. Mag., 1872.
[8] Comptes Rendus, vol. lxxxix, p. 298.
[9] Comptes Rendus, vol. lxxxviii, p. 1190. [10] Phil. Trans., 1880.

giving the wave-lengths of lines for which he derives by an extra-polation curve a position at $\lambda = 1^{\mu}{\cdot}240$, and indicating a band still beyond.

Captain Abney had previously published a map of the diffraction spectrum extending to $\lambda = 0^{\mu}{\cdot}9682$. Dr. J. W. Draper,[11] by the aid of Captain Abney's map, believes he has identified the lines α, β, γ he saw in 1842 with groups represented by Abney at $\lambda = 0^{\mu}{\cdot}8150$ to $0^{\mu}{\cdot}8350$, $0^{\mu}{\cdot}8930$ to $0^{\mu}{\cdot}9300$, and $0^{\mu}{\cdot}9350$ to $0^{\mu}{\cdot}9800$.

On our chart we have given Draper's α, β, γ according to his own locations of them. He believes these to be the same lines seen by himself, Foucault and Fizeau, and Lamansky. According to Draper, then, the lowest limit of his own or any other researches known to him in 1881, did not extend much beyond wave-length $1^{\mu}{\cdot}0000$. It appears to us probable, however, that Lamansky's lowest point was below this, and we give a copy of Lamansky's curve (fig. 3) which the reader can compare with the positions on our present charts.

3.



LAMANSKY.

These brief references concern only what belongs to our immediate purpose, and are not offered as a history of the subject.

RECENT OBSERVATIONS ON THE INVISIBLE PRISMATIC SPECTRUM.

After the return from Mt. Whitney, observations were taken at Allegheny with the train of apparatus used on the mountain just referred to, and on nearly every good day during the first six months of 1882. These were of such: 4 days in January, 8 in February, 9 in March, 9 in April, 9 in May, 12 in June. In all, 51 days.

[11] Proceedings of the Amer. Acad., 1881.

Very early the observations with this efficient apparatus (the result of improvements due to the previous two years' practice), showed by an accuracy not hitherto attained in such measures, the possibility of mapping the regions of the infra-red spectrum believed to have been first observed on Mt. Whitney, and which have remained hitherto unknown. The extreme narrowness of the bolometer thread (one-fifth of a millimeter), and the size of the prismatic spectrum employed, made it also possible, in spite of the condensation of the latter, to discriminate lines and gaps in its continuity which had escaped previous observation.* The spectrum, as the reader now sees it in the charts, was mapped with the intention of noting these interruptions of energy which had been in the previous research designedly neglected. The bolometer shows the *cold* in the principal visible Frauenhofer lines readily, but as their effect individually is slight it has not been indicated in the part of the visible spectrum above C.

The map reached approximate completeness early in April, 1882, after which date observations have been still continued daily, whenever possible, so that every portion of the curves here given, to the smallest inflection, has been observed from three to twenty times, and the accidental variations due to momentary interruptions of solar heat by invisible clouds have been, it is hoped, nearly eliminated.

The bolometric work, represented by the preceding 51 days' observations, may be here regarded as being divided into two classes having distinct though related objects.

1st. To determine the general *selective* absorption of the earth's atmosphere throughout the entire spectrum in connection with the observations already made here and on Mt. Whitney. For this purpose measurements have been made at the following deviations: 44° 30′, 45° 30′, 45° 53′, 46° 12′, 46° 30′, 46° 45′, 47° 30′, 48° 00′, 49° 00′, 50° 00′, 51° 00′.

All these points have been measured on, twice each, the whole forming a "series," and these "series" are observed at least twice daily: namely, at meridian, and when the air mass is approximately double that at meridian; or else three times daily—at meridian, when the mass of air traversed is approximately $1\frac{1}{2}$ times that at meridian, and again when it is approximately $1\frac{1}{2}\times 1\frac{1}{2}=2\frac{1}{4}$ times that at meridian.

It will be observed by reference to the map, that the points chosen for measurement coincide, as a rule, with the summits of the energy-curve, but separate investigations are still in

* Nevertheless, as the thread, however narrow, is not absolutely linear, it feels the cold before its center coincides with the center of the line. The interruptions of the energy curve are thus in fact all in a slight degree too wide, especially at the commencement of each depression, and it is probable that the bands we have marked are really due to an aggregation of finer lines.

progress on the nature of the absorption in the intervals, to determine whether the newly-observed bands are of solar or terrestrial origin. The part of the spectrum included extends from $\lambda = 0^{\mu}{\cdot}383$ (above H in the violet), to $\lambda = 2^{\mu}{\cdot}28$ (in the newly-observed infra-red region, about two octaves below Frauenhofer's B).

(As we can, in fact, obtain evidence of heat in ultra-violet waves whose length is little more than $0^{\mu}.3$, the length of the solar spectrum as now observable by the bolometer is between 3 and 4 octaves.)

The distant slit is separately exposed at each observation, and the extremity of the full swing of the galvanometer needle is read. In all these measures the galvanometer is used in the same condition of sensitiveness. The slit is opened to a constant width of 2^{mm}* (except in measuring the very feeble energy at the most refrangible end of the spectrum, where the width has been increased without prejudice to accuracy, owing to the corresponding prismatic expansion of the spectrum itself). The same bolometer is used, as a rule, having for this purpose 1^{mm} effective aperture (except in measurements at the most refrangible end of the spectrum, where the full aperture of the bolometer is used).

These observations on the absorption of different air masses, for each spectral ray, evidently furnish means for determining the curve outside the atmosphere, by the method already indicated. They also, of course, give us the means of making a map of the whole spectrum, but their use for this latter purpose is incidental.

2d. The other class of observations is for the special purpose of making a spectral map, extending from the line C to the lower limit of the infra-red.

This is carried on by means of the linear bolometer consisting of a single strip $\frac{1}{2}^{mm}$ wide. In this second class of observations, a rough map of the whole infra-red spectrum having been prepared, a very limited part of the spectrum (such as that included between 15′ of deviation) is gone over several times in the course of one day, the measurement being repeated on every single minute of arc, with a separate opening and closing of the slit, and a record made of the full swing of the galvanometer needle for each observation.

These observations are entered numerically, and corresponding charts made on large sheets of section paper. The same narrow region will thus be gone over also on different days, and the different charts subjected to a very rigid examination,

* It will be remembered that, the actual distance of the slit being 5 meters, this aperture subtends an angle little greater than one minute of arc.

so that every feature which is not common to them all is rejected or reëxamined, and in this manner the whole spectrum is studied. These original charts are on a scale four times as large linearly as that the reader now sees (Plate III).

In addition to this, on some clear days, tracings have been made upon the chart directly corresponding to the movements of the galvanometer needle; that is to say, the observer at the spectro-bolometer has moved the bolometer through the whole spectrum by means of the tangent screw; the slit has been left permanently open so that the bolometer has been constantly exposed; and the observer at the galvanometer, seeing the needle moving, as a hot or cold part of the spectrum passes over, calls the deflection of the galvanometer corresponding to each minute of arc, while a third person plots the same on section paper. In this way as many as eight curves like those here given of the prismatic spectrum, have been obtained between noon and sunset on one day, giving a picture of the action of the selective absorption of the atmosphere in every part of the spectrum, as the rays of the sinking sun pass through greater depths of air. This third method—very useful when, as in this case, many observations have to be taken in a short time—is nevertheless less accurate than those before described.

A careful bolometric and also optical setting is made on some well known line, usually C, at least once daily, to make sure that the adjustment of the instrument is equally accurate for the visible and invisible rays.

Description of the Apparatus.

The rays of the sun are reflected horizontally from the mirror of the large Foucault siderostat through an aperture in the north wall of the observatory, and received upon a plate with a slit, whose jaws, moving each way from the center by a micrometer screw, can be regulated so as to allow a beam of any desired size to pass. $4\frac{1}{2}$ meters from this slit, at the distance of its principal focus, is a collimating lens, L, of a special kind of flint glass, which has been found nearly transparent to all the invisible rays measured. This lens and the slit are fitted into opposite ends of a tube, T, $4\frac{1}{2}$ meters long, held by suitable y's. The beam of rays from the slit, now rendered parallel by the collimator, next falls upon a prism,* P, of the same kind of glass as the lens, supported on a circular adjustable

* This prism, whose optical properties are in every way excellent, was made by Mr. A. Hilger, of London. Its principal constants are as follows: size of polished faces, 53^{mm} by 49^{mm}; specific gravity, 2·895; refracting angle, 62° 34′ 43″; index of refraction for D, line 1·5798; index of refraction for H, line 1·6070. (A rock salt prism of nearly equal size and great purity, as well as prisms of quartz and spar, have been used to determine the absorption of the glass for each ray, visible and invisible.)

table over the vertical axis of the massive instrument we have called provisionally the spectro-bolometer.

Whatever the sensibility of the apparatus to heat, it is evident that we cannot accurately map the narrow spectral limits between contiguous heat and cold, unless we can fix their position with exactness. Especially when we consider that these rays are invisible, and that the whole process may be compared to a patient groping in the dark, does the need of an instrument which will record the precise point where a hot or cold line was felt, become obvious. This is the object of the spectro-bolometer, which, as well as other apparatus mentioned here, will be described more particularly in the account of the observations made on Mount Whitney. (It was made by W. Grunow, of New York, from the writer's design.)

Two long arms, A, A′, turn independently about the above mentioned axis, the angle between them being measured by a graduated circle with two verniers reading to 10″. One of these arms is directed toward the slit, and the other toward the spectrum formed by the light on leaving the prism. This latter arm carries at its extremity a concave mirror, M, of 98 centimeters focus, and on either side of the prism an accurately planed track directed toward the center of the mirror, on either of which slides a carriage with y's. Into these y's, at B, drops either of two "ebonite" cylinders, one containing the bolometer, and the other the ordinary reticule and eye-piece. The bolometer used in the measurements for these maps exposes to the spectrum a single vertical strip of platinum, $\frac{1}{5}^{mm}$ wide, covered with lampblack, and placed accurately in the axis of the ebonite cylinder by reversal under a compound microscope. The eye-piece also has its cross-wires centered in the second cylinder, and serves to examine optically the place which will be occupied by the bolometer strips when the bolometer cylinder is in the y's. The optical axis of the mirror, M, exactly bisects the angle between the direction of the arm, A′, and the central line of the track, so that a ray falling on the center of the mirror from the center of the instrument at P, after reflection falls upon the bolometer strips. C, C′ are counterpoises to offset the weight of the arms A, A′.

To adjust the apparatus for observation, the screws at D are loosened, the prism removed, and the arm A′ brought around in line with the long tube. The eye-piece being placed in the y's at B, the image of the distant slit is brought upon the central wire, when the reading of the divided circle should be 0° 00′ 00″, indicating a deviation of zero. The arm is then moved to one side as in the figure, until the mirror intercepts the rays from the prism, which has first been replaced upon its table and adjusted by the screws below. The prism is now carefully

set to minimum deviation (usually for the D_1 line) and is then automatically kept in minimum deviation for all other rays by the tail-piece and attachment at *D*. When the cross-wires of the eye-piece are set upon the *D* line the circle should indicate a deviation of 47° 41′ 15″. A bright and pure image of the spectrum about 6^{mm} wide and 640^{mm} long between the A and *H* lines is now formed in the principal focus of M near the prism, and the bolometer case being substituted for the eye-piece, the carriage is slid along the track until the central strip, placed vertically and parallel to the Frauenhofer lines, comes exactly into focus. The heat of the solar rays in any part of the spectrum may now be measured by the bolometer (the galvanometer giving a marked deflection as it passes over the leading Frauenhofer lines), and the deviation for that part is exactly indicated by the divided circle.

The galvanometer used in connection with the bolometer is a Thomson reflecting astatic galvanometer of about 20 ohms resistance, constructed especially for the purpose by Elliot Bros. of London. It is placed upon a pier entirely disconnected from the building. The scale is cylindrical, with divisions 1^{mm} apart on a transparent surface, and is placed 1 meter from the galvanometer mirror. Since the whole deflection ordinarily employed does not exceed 5°, as a rule the reading of the galvanometer requires no reduction for our present purpose. A resistance box forming the Wheatstone's bridge, and other electrical adjuncts of the bolometer are on the right of the galvanometer pier. The rheostat is in a convenient position near the scale, and the battery galvanometer for measuring and regulating the strength of the current used, is on a pier in another part of the room.

In conducting the measurements for mapping the spectrum, one observer is usually at the spectro-bolometer to set the circle to the deviation required, to see that the light from the Siderostat falls properly upon the prism, and to admit the sunlight at a given signal by means of cords attached to a sliding cover in front of the slit where the sun's rays first enter the room. Another observer, placed at the galvanometer, reads the corresponding indications of the instrument, and a third enters them in form in a book prepared for the purpose, and gives the signal for exposure. As all these observations are carried on in a partially darkened room, a fourth person is usually stationed without to wind the siderostat clock, and to give notice of any passing clouds to those within the building.

Example of the mode of observation.

As an example of the first class of measures, let us consider the observations made with the Hilger Prism on June 22, 1882. The high sun observation was made at $0^h\ 15^m$. The sun's zenith distance at this time was 17° 10′; the air mass* was 1·047 times the mass overhead; the height of the barometer corresponding to the air mass overhead was 7·39 decimeters, consequently the air mass for a zenith distance of 17° 10′ was $7{\cdot}39 \times 1{\cdot}047 = 7{\cdot}74$ decimeters.

The sun's zenith distance at $6^h\ 25^m$ (the time of the second observation,† was 79° 8′); the height of the barometer was the same as at noon, and the air mass by the same formula was 5·18 times that overhead, or $7{\cdot}39 \times 5{\cdot}18 = 38{\cdot}27$ decimeters, so that the mass of air traversed in the second observation exceeded that in the first by an amount capable of supporting 30·53 decimeters of mercury.

The galvanometer deflection obtained in the part of the spectrum whose deviation is 44° 30′ (a part which is near the extreme lower limit of the present observations, far below the visible red) was at noon 17, and in the afternoon 11. In the violet, where the deviation is 50° 00′, the corresponding deflections were 4·5 and 0·39. Let us take these two feeble extreme rays as types with which to illustrate our process. Considering first the infra-red ray we have, deflection at noon $= d_1 = 17$, deflection in afternoon $= d_2 = 11$, difference in mass of air traversed $= M_2\beta_2 - M_1\beta_1 = 30{\cdot}53$ decimeters, which, by its absorption, has produced the difference in the deflections. t representing the amount of energy transmitted by a layer of air equivalent to 1 decimeter of mercury, we find from the formula

$$t = (M_2\beta_2 - M_1\beta_1)\sqrt{\frac{d_2}{d_1}}$$

$t = {\cdot}986$; that is, a mass of air capable of supporting 1 decimeter of mercury in the barometer, transmits 98·6 per cent of the energy of this particular kind of ray. This quantity t we call the coefficient of transmission of the ray.

Knowing now the amount of energy transmitted by one such layer of air, we can find the amount transmitted by the 7·74 layers which intervened between the observer and the sun at noon, namely $\cdot 986^{7{\cdot}74} = {\cdot}895$. Only 89·5 per cent, therefore, of the original unknown heat of the ray, which we will

* Computed from the formula $M = \frac{0{\cdot}0174 \text{ Tabular Refraction}}{\text{cos. app. altitude.}}$

† In general it is not advisable to make observations at so great a zenith distance as this.

represent by E, reached the observer at noon, producing a deflection of 17, or ·895 E = 17, giving

$$E = \frac{17}{\cdot 895} = 19\cdot 0$$

That is, had our instrument been placed outside the atmosphere at that time, it would have indicated a deflection of 19 instead of 17.

By a similar process we find that the coefficient of transmission for the violet ray is ·923, from which we see that the ultra-red ray is transmitted with greater facility than the violet. The amount of this violet radiation transmitted by the whole depth of atmosphere at noon was ·538, from which its energy outside the atmosphere was $\frac{4\cdot 5}{\cdot 538} = 8\cdot 4$.

The table below gives the coefficients of transmission, etc., for these and other points in the spectrum where measurements were taken on this day. The first column gives the deviation of the observed ray in the spectrum of the prism used, the second and third columns the deflections obtained with the galvanometer at noon and in the afternoon, respectively, the fourth column the coefficient of transmission (for an atmosphere supporting one decimeter of mercury), the fifth the transmission of the whole depth of atmosphere at noon, obtained by raising the coefficient of transmission to the 7·74 power, and the last the computed energy outside the atmosphere expressed in galvanometer deflections.

TABLE VIII.

Deviation.	d_1	d_2	t	$t^{M,\beta,}$	E
53° 00′	0·02	0·00			
52 00	0·21	0·00			
51 00	0·96	0·09	·925	·549	1·8
50 00	4·5	0·39	·923	·538	8·4
49 30	7·3	----	----	----	----
49 00	13·	3·0	·953	·689	18·9
48 00	43·	12·5	·960	·731	58·8
47 30	72·	38·	·979	·850	84·7
46 45	158·	109·	·988	·910	173·7
46 12	209·	134·	·986	·894	233·8
45 53	175·	107·	·984	·883	198·3
45 28	122·	79·	·986	·895	136·2
44 30	17·	11·	·986	·895	19·0

Similar reductions have been made for each day's observation the result from each being confirmatory of the statement here (see column $t^{M,\beta,}$) that the atmospheric absorption *diminishes* continuously as the wave-length increases (save for the interruptions already cited) to the extremity of our charts.

The graphic representation of this and other extra telluric curves of energy will be given in a later memoir, in such a

form as to show from the mean of a year's observations, the percentage of absorption suffered by each ray in the entire spectrum, visible and invisible.

The reader who may desire still fuller details as to the apparatus, the original observations and their treatment, is referred to the forthcoming official publication already mentioned. In the later memoir will be found a description of the method used for determining the wave-lengths corresponding to measured deviations, and the formulæ for deducing from the prismatic spectrum, the distribution of the energy and the extent of the spectrum on the normal scale.

Summary.

As one result of this present research, the chart of the prismatic spectrum as observed at Allegheny with the bolometer is now presented (Plate III). The abscissæ are proportional to deviations and the ordinates to measured energies. The second chart now given, (Plate IV), represents the normal spectrum as deduced from the prismatic, as it has been thought advisable to present it here for the reader's convenience, in advance of a description of the means used for making it. The abscissæ on this are proportional to actually measured wave-lengths, and the ordinates to measured energies. In both charts, the area between ordinates corresponding to like wave-lengths is the same, and hence the total areas are the same. Their very dissimilar contour is due to the prismatic distortion.

Faint indications of solar energy below the lowest point here shown, have been found, and these, with some considerations as to the nature of the new absorption bands, may be given hereafter, together with tables (already prepared) of the absorptive action of the *solar* atmosphere for each spectral ray. These will, it is hoped, give with a satisfactory approximation the distribution of the energy, before any absorption whatever; at the source, that is, of the energy, in the photosphere itself.

The extent of the newly observed region may be most clearly seen by reference to the map of the normal or diffraction spectrum. (Plate IV.) Previous maps end at or near wave-length $1^{\mu}{\cdot}2$. Beyond this point (with the exception of the single band near wave-length $1^{\mu}{\cdot}4$) every line, and every ordinate representing heat, is believed to be new. The extent of the region here newly mapped, is then considerably larger, on the normal scale, than the whole of that (both visible and invisible) previously known.

We observe that the prismatic spectrum is enormously expanded at the violet end. To carry this on the prismatic

scale to wave-length $0^{\mu}{\cdot}3$ would extend it far beyond the limits of our chart. *All* the actual energy in the entire ultra-violet part is, however, insignificant—how insignificant can best be seen by reference to the normal chart, where the minute area beyond wave-length $0^{\mu}{\cdot}4$ represents the *whole* ultra-violet energy.

We are accustomed to speak of the ultra-violet or infra-red regions without reflecting on the enormous difference between their actual importance. The reader will be able to see by a simple inspection of the normal chart and a comparison of the little area above wave-length $0^{\mu}{\cdot}3$, and the great area below wave-length 0·7, that the latter is nearly a hundred times as great as the former. Yet the former, owing to the prismatic expansion, and to the selective absorption by the feeble rays of this region of certain salts of silver, with which it can be photographed (while the far greater luminous energy below makes little impression on these salts), has occupied more attention than the latter. When we observe here how the infra-red region is compressed by the prism, we can understand how its extent has been under-estimated. Its real extent is so vast that we should accustom ourselves to consider "in the infra-red region" as a wholly vague term, needing to be supplemented with a description of the particular part of the infra-red referred to.

It may be well to epitomize the principal results of all these researches as far as they have been here given. In general they emphasize and extend our first conclusions.

1st. In measures now made for the first time on approximately homogeneous rays in the diffraction spectrum, we find that the maximum energy is above the red and is placed in fact near the yellow. The place of this maximum point varies with the sun's altitude, ranging from a wave-length of nearly $0^{\mu}{\cdot}55$ on a clear day and with a high sun, to a wave-length of $0^{\mu}{\cdot}65$, or even more before sunset. On the normal scale then, the position of the maximum of heat in the spectrum does not vary widely from that of the maximum of light. It is shown later how similar results are deducible from the prismatic spectrum.

2d. By comparing the ordinates for high and low sun in different parts of the spectrum, we see that they grow unequally, indicating an enormous systematic absorption, increasing toward the ultra-violet, and diminishing toward the infra-red, and these ordinates not only indicate its character but give its amount. In contradiction to the statement of many investigators and of present opinion on the point, we find that (according to these measures) the absorption grows on the whole less and less, as we go down below the red, to a point

near wave-length $2^{\mu}\cdot 8$. By this it is not meant to deny the existence of regions of very great local absorption in the lower spectrum. These same observations do in fact point out new regions of such local absorption. But excepting these they warrant us in saying that, broadly speaking, the absorption through the whole spectrum, visible and invisible, appears to follow one simple law, and to decrease where the wave-length increases; so that not only is the ultra-violet more absorbed than blue, blue than yellow, and yellow than red, but that red is more absorbed than the infra-red, and each degree of infra-red is more so than the next one below it.

3d. By the use of the ordinary logarithmic formula, here employed in its legitimate application to homogeneous waves, we can pass from the curve inside to that outside the atmosphere. In other words, we can virtually transport our observing station to a point wholly above the air, and determine the distribution of the sun's heat before this unequal absorbent action of our atmosphere has affected it. We need only embody the results for selective absorption given by our tables in a simple graphic construction (like that here shown in connection with the preliminary investigation), to see that the point of maximum heat *outside* our atmosphere lies near wave-lengths 0·50 to 0·55; or, as we are entitled to say, that the hottest portion of the spectrum outside the atmosphere will be found rather in the green than as here near the yellow.

It is probable, from our measurements, that the sun would appear of a decidedly bluish tint to the naked eye placed without our air.

This atmosphere which we are so accustomed to regard as colorless, has then, in fact, played a part analogous to that of a yellowish or reddish glass whose impure color is not a monochromatic yellow or red, but a compound of all spectral tints in unaccustomed proportions. Had we in all our lives had no light but from an electric light, seen through such a reddish glass shade, we should probably have believed this reddishness to be the "natural" or proper color of the naked carbons, and moreover that it represented "the sum of all radiations." It would apparently answer in an individual brought up in ignorance of any other light, to our common notion of *whiteness*, so that even though it really possessed color, the medium would (previous to investigation) be deemed colorless. In the same way common opinion regards our air as colorless, yet it cannot be so, but must necessarily (according to these observations), be considerably colored.

As we have been accustomed to regard it as colorless, however, it is clear that were it removed we should, in seeing the sun's true appearance for the first time, regard the sun itself as colored.

Our white light, then, is *not* the sum of all radiations, but only of a part, even of the visible ones.

4th. We can, by measuring the area of the curve outside our atmosphere and comparing it with the area of the curve within, obtain by a method never before pursued, which is in close accord with theory, a value for the Solar Constant.

Previous observations have found from 1·7 cal., in the time of Pouillet, to 2·5 cal. in that of Violle, with a tendency to increase. The value here given from our preliminary investigation is 2·84 cal. The last figure is of little weight and the exactness of that in the first decimal place is probably open to doubt. The conclusion which we are entitled to draw from these investigations in the stage here presented, is that the Solar Constant is in reality greater than has been heretofore supposed, and that it is probable that it is not very greatly inferior to 3 calories. This important point will be discussed fully in connection with the Mt. Whitney observations, with which the complete graphical constructions elucidatory of our present tables will be given.

5th. These observations show heat in extreme ultra violet rays, and the change of temperature (hitherto unobserved), in the Frauenhofer lines. They lend increased probability to the belief that *all* the energy in any ray can be exhibited as heat, if there be a proper medium to receive this energy. Their evidence, so far as it goes, then, favors the conception of one solar energy which is interpreted in terms of heat, or of light, or of chemical action, according to the medium by means of which we choose to observe it.

6th. The *ratio* of luminous to dark heat has evidently been wholly changed by the selective absorption. The ratio at the sea-level may be found with close approximation by measuring the two areas, (1st) above the point where we assume the luminous spectrum to end, and (2d) below it. This point each one may define differently, for the extent of the luminous spectrum depends much upon our precautions for observing it. If we assume it to end near B, then three-quarters of the energy must be termed invisible; if at the actual visual extremity (far below A), then less than half. To fix our ideas, let us suppose it to terminate at Frauenhofer's A. We then find

luminous and ultra-violet energy (within the smooth curve)	0·368
infra-red energy	0·632
	1·000

The ratio of the invisible (infra-red), to the whole then is 0·632, and there is reason to believe this value rather too small than too large. If, however, we deduct the space occupied by the gaps in the lower spectrum the ratio becomes 0·562. The

infra-red energy at sea-level may be roughly taken, as thus defined, at three-fifths the whole. At the same time the ratio of luminous to obscure energy without our atmosphere is, we repeat, far greater than within it.

We conclude (among other consequences of our observations) that since the heat in the shorter wave-lengths (corresponding in a general sense to high solar temperature) was thus relatively greater before absorption, we are obliged to increase our usual estimates, not only of the amount of heat the sun sends us, but (and very greatly) of the effective *temperature* of the solar surface.

The relatively small amount of energy, corresponding to great wave-lengths in the infra-red, is due not so much to absorption as to the fact that there is no considerable solar energy existing there at all. The relatively great amount of energy in the luminous part of the spectrum exists there, not on account of a feeble absorption, but *in spite* of a strong absorption, and the original solar energy here was even much more considerable than what we see.

It is probable, however, that the solar spectrum before absorption, though originally weak below the red, *yet extended very much farther into the infra-red than our charts indicate.* We may even regard it as probable that some agent of the atmosphere acts as an almost complete barrier to the entrance or departure of rays, below the point charted.

It should be understood that these researches have here a practical bearing of great consequence. The temperature of this planet, and with it the existence, not only of the human race, but of all organized life on the globe, appears, in the light of the conclusions reached by the Mt. Whitney expedition, to depend far less on the direct solar heat, than on the hitherto too little regarded quality of *selective* absorption in our atmosphere, which we are now studying.

The discussion of these and other points is reserved for a subsequent memoir. Among these will be the fuller consideration of the place of the principal absorption of water vapor, a consideration which it will be advantageous to present in another connection. It is to be remembered that all the values here given are presented as approximate and not as final ones.

In presenting these researches, on the part of the Allegheny Observatory, I should state that the considerable especial expenditures they have involved have been met by the generosity of a friend of that institution, whose aid, which alone made them possible, I would gratefully acknowledge.

In conclusion I desire to say that I have been aided throughout them by Messrs. F. W. Very and J. W. Keeler of this Observatory, with an efficiency and interest in their prosecution without which they could hardly have taken their present form.

Allegheny Observatory, Allegheny, Penn., Dec. 30, 1882.

ART. XIX.—*New locality of the Green Turquois known as Chalchuite, and on the Identity of Turquois with the Callais or Callaina of Pliny;* by WILLIAM P. BLAKE.

IN this Journal,* March, 1858, I directed attention to the occurrence in New Mexico of a green turquois highly prized as a gem by the aborigines and known as "*Chal-che-we-te.*" The completion of the railway along the valley of the Rio Grande has made the Cerillos Mountains, in which the gem occurs, much more accessible than it was, and the ancient mine has been re-opened and worked to some extent by Eastern capitalists, as made known by Professor Silliman.† The stone is in consequence more abundant than before, and at Wallace Station on the railway very good specimens can frequently be obtained of the Pueblo Indians.

I have recently visited another locality where chalchuite occurs and was mined by the ancients. This is in Cochise County, Arizona, about twenty miles from Tombstone, in an outlying ridge or spur of the Dragoon Mountains and not far from the stronghold of the Apache chief, Cochise, so long the terror of that region. This elevation is now known as the "Turquois Mountain," and as there are several deposits of argentiferous ores near it, a mining district has been formed called the "Turquois District."

At the turquois locality there are two or more ancient excavations upon the south face of the mountain, and large piles of waste or debris thrown out are overgrown with century plants, yuccas and Cactaceæ. It has not been worked for a long time and probably never by the Apaches. The excavations are not as extensive as at Los Cerillos, and it is more difficult to find specimens of the mineral. It is evidently much less abundant than at the New Mexican locality. Enough of the gem was obtained, however, by searching in the waste heaps, to show that it is identical in its appearance with the New Mexican chalchuite. The rock is also similar and the chalchuite occurs in seams and veinlets rarely more than an eighth or a quarter of an inch in thickness.

The color is light apple-green and pea-green, precisely that of the New Mexican stone, as generally seen. There is in some fragments a faint shade of blue as at Los Cerillos, but the true normal color appears to be green rather than blue.

The specific gravity I find to be, of two different fragments, 2·710 and 2·828. The first was slightly porous and earthy and the second dense, hard and homogeneous. These results are

* The Chalchihuitl of the ancient Mexicans, this Journal, II, xxv, 227.

† Ibid., III, xxii, 67, July, 1881.

higher than I obtained with the specimens from the surface at the New Mexican locality, viz: 2·426 to 2·651. Two determinations recently made gave 2·500 as the specific gravity of two partly cut stones from the old Cerillos locality.

This stone is peculiarly interesting archæologically. I have shown, in my former paper, that it was in general use and high esteem among the Aztecs and Moctezumas, before the advent of the Spaniards, and that the Pueblos and Navajoes still value it highly. It is evident that the stone was known to all the leading tribes inhabiting the plateau region of Mexico, including the northern portion now known as Arizona and New Mexico. Of this there is much confirmatory evidence, obtained since my first communication. The reopening of the old mine revealed many implements of the stone age, and showed workings of much greater extent than even the enormous surface excavations indicated. The early explorers and historians of New Spain chronicle the fact that the inhabitants of Cibola had an abundance of turquoises. We also know that the chalchuite was worked with considerable skill by ancient lapidaries. A few years ago a remarkable specimen of neatly executed mosaic work in chalchuite was dug up from the ruins near Casa Grande on the Gila. A mask from Mexico preserved in the British Museum appears to be overlaid with small tablets of the same green gem. The stone thus appears to have been used for incrusting and overlaying very much as it was in ancient Persia. Its opacity, hardness and the tabular form in which it is obtained, fit it peculiarly for this use. The descriptions by Pliny of the *Callais* or *Callaina* (*Καλλαϊνὸς λίθος*) apply well to a stone like the chalchuite. According to C. W. King* in his admirable work on gems, *callaina* probably was the original reading in the chapter of Pliny treating of callais, and is a name derived from a peculiar green dye, the *callaicum*. Pliny describes the color of the callais or callaina as pale yellow, mixed with green, and again that the best have the color of the emerald. . . "No gem is more improved by setting in gold, and gold itself is better set off by no gem. The better kind lose their color by wetting with oil, grease or wine, but the inferior retain it more permanently." But Mr. King while noting the fact that the callais of Pliny is now universally understood by modern mineralogists to have represented our turquois, thinks that the identification is not borne out by the description which Pliny gives. It seems to him to indicate a *transparent* rather than an opaque stone, an inferior peridot, perhaps. Pliny's statement that the stone is found of remarkable size, but full of holes and dross (*fistulosa*), is considered as repugnant to the idea of an opaque solid body like the modern turquois.

* C. W. King. The Natural History, Ancient and Modern, of Precious Stones and Gems, p. 136.

Such objections vanish before the specimens of chalchuite as they come from the mine. Large masses do not occur without earthy impregnations and mixtures of a yellow color. The mode of origin and the accretion of more or less rounded semi-globular crusts in the cracks of the rock often leave spaces in a fistulous form, and these are generally filled by earthy deposits, colored by iron oxide. In the selection of masses for jewels such imperfections are cut away and of course do not appear in the turquois of commerce.

That the stone of which Pliny treats is not a transparent gem seems to be clearly shown by the statement of the effect of oil or grease upon it. He evidently is describing an absorbent stone, a porous and consequently opaque mass. It is an interesting fact that the Pueblo Indians at this day resort to the expedient of soaking the chalchuite in tallow or grease to heighten the color and to make the tint of the larger specimens more uniform. The grease is taken up by the more porous and softer parts of the stone, while the harder portions of a deeper tint do not absorb it. Ben Mansur, in describing the turquois, also refers to the improvement of the color of some sorts of the stone by steeping in oil. Damour* refers the green-colored hydrous phosphate of alumina ornaments, found in a Celtic grave, to the callais of Pliny, especially in view of the green color. The mineral is evidently a somewhat altered green turquois and is not specifically different, but is well entitled to the name callainite, the modified form of Pliny's name proposed by Professor Dana.† The name turquois is certainly unsatisfactory for the species. Callais or callaina and also *chalchuite*, not only have precedence in time, but are distinctive and better in form, and with the modification noted are in accord with the terminology of the nomenclature of the science. Moreover they are not misleading as turquois (Turkish stone) is in regard to the source of the gem. Although some of the mineral may have been found in Turkey, Persia has been the chief locality. This is fully recognized by Buffon,‡ and he cites from various authorities particulars regarding the localities in Persia, at Nichabour and at Firusku in the province of Erak. One of these authors§ says that the turquoises were there called "*firuses*," and King mentions *Firuzegi* as the name of Persian turquois. Brongniart, ii, 225, quoting from Chardin, says the turquois is found in a mountain named "*Phirous*," between Hircania and Parthide. Again the name turquois is applied to the blue fossil bone or ivory, called also the Occidental turquois and odontolite, of which large quantities were formerly, and are still, used in jewelry.

* Compt. Rendus, lix, 936. † Dana's Mineralogy, 5 edit., p. 572.
‡ Buffon, Histoire naturelle des Mineraux, 1790, viii, 51.
§ Adam Olearius; Voyage, &c., Paris, 1656, p. i, 461.

The fact that the beautiful green of chalchuite is the normal color of the gem, and that among the ancients, and even so recently as the last century, the stones of a green tint were the most prized, should lead to a higher appreciation of this gem and to its more extended use in jewelry and choice mosaics. Mr. C. W. King, in the work already cited, says that the very rare antique works in turquois are in the green kind, notably the head of Tiberius at Florence in a stone as large as a walnut, and other half relief works in green turquois in the Marlborough collection. The chalchuite is well adapted to cameo or intaglio work, and the color is finer by candle light than by sun light and the blue tint is deeper.

In using the name *chalchuite* instead of the longer form "*chalchihuitl*," as in my former paper, I now follow Bernal Diaz, the historian of the conquest of Mexico, rather than Lockhart, the translator, for brevity and the accordance of chalchuite with the now generally accepted terminology of the names of mineral species. This name in all probability is older than Pliny's, and if not given the precedence of callainite, should at least have the second place in the synonymy of the species.

Mill Rock, New Haven, Conn., Jan. 6, 1883.

ART. XX.—*On portions of the Skeleton of a Whale from gravel on the line of the Canada Pacific Railway, near Smith's Falls, Ontario;** by J. W. DAWSON.

BONES of large whales are not of infrequent occurrence on the less elevated terraces of the Pleistocene period on the Lower St. Lawrence. I have seen them at several places in the neighborhood of Metis, on the lowest sea terrace, now elevated only a few feet above the level of the sea, and they are reported to have been found on the second terrace at an elevation of 60 to 70 feet. Mr. Richardson, late of the Geological Survey, informs me that he has seen them in several other places on the lower terraces. It has also been reported that bones of a whale were found on Mt. Camille, in rear of Metis, at a considerable elevation; but Mr. Richardson, who visited the locality, failed to verify the statement. The bones found on the lower, and therefore modern terraces, are usually in a good state of preservation, and have a very recent appearance. The above statements relate to remains of the larger whalebone whales.

* From the Canadian Naturalist; communicated in an advanced proof by the author.

Remains of the *Beluga*, or small white whale, were found by the late Dr. Zadock Thompson, author of the "Natural History of Vermont," in the marine clay in the township of Charlotte, Vermont, at an elevation of 150 feet above the sea. They were associated with shells of *Saxicava* and *Leda*. The species was supposed to be distinct from the *B. Catodon* Gray, and was named by Thompson *B. Vermontana*. I have found detached bones of *Beluga* in the Post-pliocene clays of Riviere du Loup, and considerable portions of a skeleton were found in the excavations for the Intercolonial Railway, on the south side of the Baie des Chaleurs, and were described by Gilpin in the Transactions of the Nova Scotia Institute of Natural Science.* Bones have also been found in the brick clays near Montreal, and a specimen was discovered several years ago in sand holding *Saxicava*, near Cornwall, Ontario. The last named specimen was studied by Mr. Billings, and its bones compared with those of the modern species in the McGill College Museum. On this evidence Mr. Billings concluded that it belonged to the modern species, and I believe extended this conclusion to Dr. Thompson's specimen; the distinctive characters of which, as stated by that naturalist, seem not to exceed the individual differences in modern specimens.

But though the *Beluga*, which now extends its excursions far up the St. Lawrence, and has even been captured in the vicinity of Montreal, occurs as far west as Cornwall; no remains of the larger whales have, so far as I am aware, been found so far inland, until the discovery of the specimens referred to in the present note. These were found, as I am informed by Archer Baker, Esq., General Superintendent of the Canada Pacific Railway, "in a ballast pit, at Welshe's, on the line of the C. P. Railway, three miles north of Smith's Falls, and thirty-one miles north of the St. Lawrence River, in the Township of Montague, County of Lanark. They occurred in gravel at a depth of 30 feet from the surface, and about 50 feet back from the original face of the pit."

Mr. Peterson, C. E., has been kind enough to obtain for me the elevation of the place where the remains were found, as indicated by the railway levels. It is 420 feet above the level of the St. Lawrence at Hochelaga, or as nearly as possible 440 feet above sea-level. It is interesting to observe that this corresponds exactly with the height of one of the sea terraces on the Montreal mountain, and is only 30 feet lower than the well-marked beach with sea shells above Côte des Néiges, on the west side of the mountain. The highest level at which Post-pliocene marine shells are known to occur on Montreal mountain is near the park-keeper's house, at an elevation of

* Volume ii, 1874.

about 520 feet. These marine deposits of Montreal are of the same geological period with the Cetacean remains in question, so that the animal to which these belonged may have sailed past the rocky islet, which then represented Montreal mountain, at an elevation of 400 feet above the lower levels of the city, and in a wide sea which then covered all the plain of the Lower St. Lawrence.

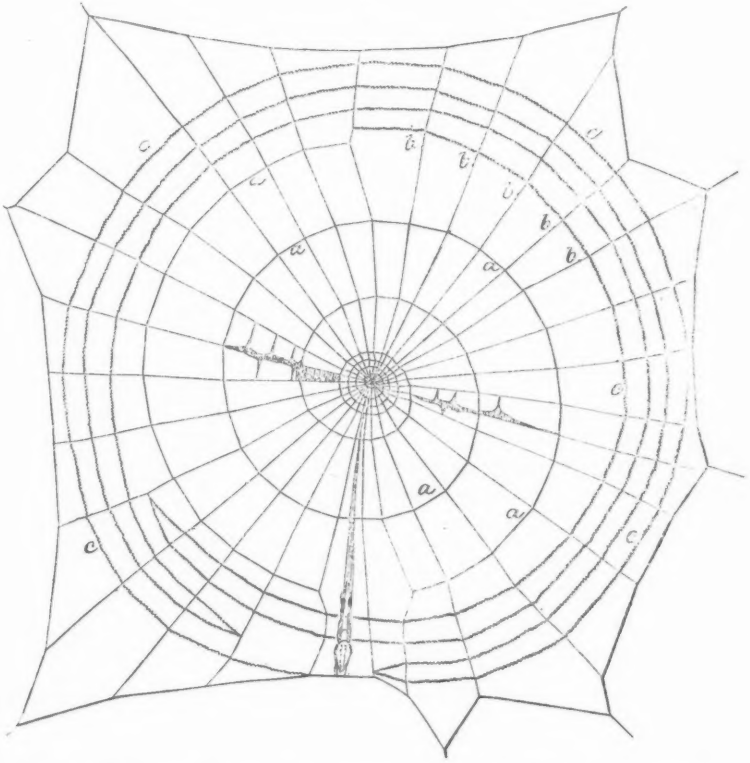
The deposit in which the remains occurred is no doubt the equivalent of the Saxicava sand and gravel, and was probably a beach or bank near the base of the Laurentian hills, forming the west side of a bay which then occupied the Silurian country between the Laurentian hills north of the Ottawa, and those extending southward toward the Thousand Islands, and which opened into a wide extension of the Gulf of St. Lawrence, reaching to the hills of Eastern Canada and New England, and westward, perhaps, to the Niagara escarpment at the head of Lake Ontario. Such a sea might well be frequented by whales in the summer season, and individuals might occasionally be stranded on shallows, or driven ashore by gales or by the pressure of floating ice.

The bones secured consist of two vertebræ and a fragment of another with a portion of a rib, and others are stated to have been found. They are in good preservation, but have become white and brittle through the loss of their animal matter. On comparison with such remains of whales as exist in the Peter Redpath Museum, and with the figures and descriptions of other species, I have little doubt that they belong to the Humpback whale, *Megaptera longimana* of Gray, *Balaena boops* of Fabricius, a species still common in the Gulf of St. Lawrence, and which extends its range some distance up the river, and is more disposed than most others of the large whales to haunt inland waters, and to approach the shores. I have seen it as far up the river as the mouth of the Saguenay, and there is reason to believe that occasionally it runs up much further. It is a species well known to the Gaspé whalers and often captured by them. Of course with so little material it is not possible to be absolutely certain as to the species, but I think it may safely be referred to that above named. The larger of the two vertebræ, a lumbar one, has the centrum eleven inches in transverse diameter, and is seven inches in length. The smaller, a dorsal, is ten inches in its greater diameter, and four in length. Through the kindness of Mr. Baker the specimens have been deposited in the Peter Redpath Museum of McGill University.

ART. XXI.—*The Cobwebs of Uloborus;* by J. H. EMERTON.

IN a recent article on the cribellum and calamistrum (Archiv für Naturgeschichte, 1882), P. Bertkau has cleared up the uncertainty about the structure of the cribellum by finding again the secreting glands at the ends of the fine tubes which

1.



have their outlets in this organ. Like Blackwall, Bertkau places together in one group all the spiders which are provided with the cribellum, instead of dividing them among several families according to their form and habits, as has been done by more recent writers. One of the principal reasons for this division has been the supposed resemblance of the webs of *Hyptiotes*, and especially of *Uloborus*, to those of the *Epeiridæ*.

and Bertkau appears to have had little opportunity to study the spinning habits of these two genera. He quotes the account by Wilder, of the making of the web of *Hyptiotes*, in Proc. Am. Ass. for Adv. of Science, vol. xxii, but is led into the mistake of supposing that the web of *Uloborus* is also a sector of a circle made in some such way as that of *Hyptiotes*. This is, however, by no means the case. The web of *Uloborus* is as round as that of most *Epeiridæ*, and is made in the same way by spinning a number of threads radiating from a center, cross-

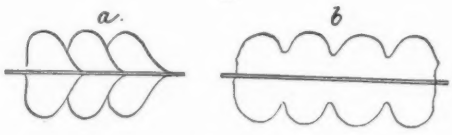
2.

ing these with a loose spiral of the same kind of thread, and afterward beginning at the outside, crossing the rays again with closer spirals, gradually removing the first spiral of smooth thread, leaving only slight thickenings of the rays to show where it was attached. The principal difference between the webs is in the structure of the thread of the final spirals. In *Epeira* it is covered by a viscid liquid that collects on it in drops. In *Uloborus* it is covered by a band of fine threads drawn out by the calamistrum from the cribellum as described in *Hyptiotes*.

Fig. 1 represents an unfinished web of *Uloborus walckenærins* seen in France, showing the central part still occupied by the preliminary spirals, *a a*, while the outer part is covered with curled threads, and the smooth spirals cut away leaving thickened spots, *b b*, on the rays. In the finished web most of the spirals pass regularly around, but the outer ones are often more or less irregular as in *Epeira* webs, according to the shape of the space in which the web is made.

After laying her eggs, this spider, like many others, becomes careless about her web, and repairs it only enough to keep the cocoons in place, so that many imperfect and irregular webs are found at that season. I have seen such webs made by *Uloborus walckenærius*, and the only web I have seen of the American *Uloborus=Phillyra riparia* Hentz (fig. 2), is imperfect from the same cause, but is evidently the remains of a nearly round web, the rays meeting somewhat nearer the upper than the lower edge.

3.



The thread of *Hyptiotes* and *Uloborus* has a strong smooth thread through the center. That of *Hyptiotes*, which I have examined fresh, has the finer part arranged in regular loops or scollops (fig. 3), *a*, *b*, in which the separate fibers cannot be distinguished. The thread of *Uloborus*, at least when old and dry, has the loops longer and less regular, and I have not been able to distinguish the separate fibers except at the edges of the band.

The close resemblance of the web of *Uloborus* to those of the *Epeiridæ* makes the classification of this genus still more difficult, for while its structure shows its close relationship to *Hyptiotes* and the other *Cinyflonidæ*, it is highly improbable that the habit of making such complicated webs of the same kind should have been acquired separately by *Uloborus* and by the *Epeiridæ*.

Art. XXII.—*Glacial Drift in the Upper Missouri River Region;* by Charles A. White.

[Published in advance by permission of the Director of the U. S. Geological Survey.]

During my summer's work for the U. S. Geological Survey upon the Laramie Group, in the region of the lower portion of Yellowstone River in Montana, I was able to make some observations upon the Glacial Drift which are an interesting addition to our knowledge of its limits in the region west of the Mississippi River. Professor T. C. Chamberlin in his work for the Survey has traced this drift as far up the Missouri River as the town of Bismark, but the observations made by myself were much farther westward, and extended along the valley of the Missouri River for a distance of forty miles below the mouth of the Yellowstone, an equal distance above the mouth of that river, both in its own valley, and in that of the Missouri River. That is, my observations extended eighty miles along the Missouri, and forty miles up the Yellowstone. The drift material was seen mainly upon the crests and slopes of the bluffs which border those valleys, and consists of small bowlders and coarse gravel, without clays. It was found nowhere abundant, and over a large part of the space within which it was found, it is apparently absent.

The character of the rock composing the bowlders and gravel is essentially the same as that which I have seen in Northern Iowa and Southern Minnesota. The rock is mostly syenitic, some showing no lamination, some showing it indistinctly, and some being schistose; and the proportions of feldspar, quartz and hornblende, varying much in different specimens. Among these bowlders of Archæan rocks are frequent masses of cream-colored magnesian limestone containing fragments of fossils, which I regard as belonging to the Galena division of the Trenton limestone.

This glacial drift material is very different from the coarse drift gravel which is so abundant in the valley of the Yellowstone, and which has doubtless been derived from the mountain region about its sources. The latter is similar to that which is usually found in the valleys of rivers which have their rise in the Rocky Mountains.

ART. XXIII.—*Late Observations concerning the Molluscan Fauna, and the Geographical extent of the Laramie Group;* by C. A. WHITE.

THE observations that have been made concerning the faunal characteristics and geographical extent of the Laramie Group during the past year are of considerable importance. We now know the strata of that group to exist at numerous and extensive localities through more than twenty-four degrees of latitude; that is, from the State of Nuevo Leon in Mexico to the Valley of the Saskatchawan in British America.

Before giving a statement of the late observations referred to, it is necessary to an understanding of them that I should make a few explanatory remarks.

When speaking of the invertebrate fauna of the Laramie Group I refer to the fauna of a formation that is wholly different from that of any of the marine Cretaceous formations, with one of which some writers have confounded it; a fauna that as a whole and in details stands out more clearly distinct from the fauna of all the other geological formations of North America than any of the latter do from each other. The Laramie fauna contains no true marine types of any kind, but it does contain many brackish-water molluscan forms, and also the remains of many fresh-water and land mollusks. This fauna characterizes a great wide-spread geological group of strata in the most distinct and unequivocal manner, several of its molluscan species now being known to occur at localities more than a thousand miles apart.

It is cause for great regret that the admirable Text-book of Geology lately published by Professor Archibald Geikie should contain so erroneous a statement as it does of the molluscan fauna of the Laramie Group. I do not hesitate to assert that not one of the molluscan species mentioned in that statement were ever found in strata of the Laramie Group; the non-marine forms which he mentions being evidently those which were discovered by Mr. Meek in an estuary deposit of true Cretaceous age, at Coalville, Utah. Furthermore, not one of the numerous species which do characterize that group are anywhere mentioned in the book. With due recognition also of the value of the geological labors of Professor J. J. Stevenson, who has published several articles in this Journal, and in the Wheeler Reports, upon the Laramie Group, I am quite unable to reconcile his statements with my own extensive observations of that group and the study of its fossils. That any true Laramie strata ever alternate with those of the Fox Hills Group, or any other marine Cretaceous group; or that any true marine

fossils were ever collected from any strata of the Laramie Group, I cannot admit. I regard all such statements as the result of a misunderstanding of the stratigraphical geology of the region in which such observations are said to have been made.

The true molluscan fauna of the Laramie Group has been published mainly by the late Mr. F. B. Meek and myself. Most of Mr. Meek's species are figured in vol. ix. of the U. S. Geological Survey of the Territories; where they are referred to the Fort Union and Judith River groups respectively, which are now known to be only portions of the great Laramie Group. I have illustrated this fauna to a large extent in the Annual Report of the U. S. Geological Survey of the Territories for 1878,* and in the third Annual Report of the Director of the established United States Geological Survey. The latter work, now passing through the press, contains illustrations of all the known molluscan species of the Laramie Group.

In former publications I have referred to the fact that the strata of this group have been recognized from northern New Mexico to the British Possessions; and from the meridian of Great Salt Lake to that of Western Kansas and Nebraska. Besides the results of a full season's personal work upon the Laramie Group in Montana, I have, within the past year, received collections of its characteristic molluscan fossils from Professor Samuel Aughey, which he obtained in Western Nebraska; from Mr. Lawrence Bruner of the Entomological Commission; from the valley of the Saskatchawan, and from Mr. James T. Gardner from the State of Nuevo Leon in Mexico.

Those collected by Mr. Bruner were obtained in the valley of the South Saskatchawan, twenty miles above the mouth of the Bow River. They number only three species, but they are referrable to well-known Laramie forms. One of them is that which was described by Meek & Hayden from the Judith River group, under the name of *Ostrea glabra;* the second is *Corbicula* (*Leptesthes*) *fracta* Meek, the type-specimens of which came from the Bitter Creek series in Southern Wyoming; and the third is referred to *Goniobasis convexa* Meek and Hayden, the type specimens of which came from the Judith River Group.

The collection that has been received from Mexico was made at a point about seven and a half miles northwest of Lampazos in the State of Nuevo Leon, and numbers seven species, of which the following is a list:

* This volume is not yet published, but my extract from it, with 32 plates of illustrations, was published in 1880.

1. *Ostrea Wyomingensis* Meek.
2. *Anomia micronema* Meek.
3. *Modiola regularis* White?
4. *Corbula subundifera* White.
5. *Corbicula cytheriformis* Meek and Hayden?
6. *Odontobasis buccinoides* White.
7. *Melania Wyomingensis* Meek.

This Mexican collection, so far as it goes, is an almost exact duplication of the Laramie molluscan fauna of the Bitter Creek series as found at Rock Springs, Point of Rocks and Black Buttes, in Southern Wyoming, points which are more than a thousand miles north of the Mexican locality. The specimens represented by No. 1 are unmistakably like those from the original locality at Point of Rocks. Those of No. 2 are small examples such as are found at Rock Springs. The specimens of No. 3 are merely fragments, but they are presumably identical with *Modiola regularis* White, which occurs at Rock Springs. Those of No. 4 do not vary perceptibly from the type-specimens, which were found at Point of Rocks.

The *Corbicula* of No. 5 is closely like the form which I have referred to *C. cytheriformis*, found at Point of Rocks, but it is a little more angular upon its posterior slope. No. 6 cannot be separated from the type specimens of *Odontobasis buccinoides* that were found at Point of Rocks. The specimens of *Melania*, No. 7, present some variation from the type specimens that were found at Black Buttes, consisting mainly in their shorter form and the more pronounced character of the ornamentation. They are, however, regarded as of the same species, and their variation from the type specimens is no greater than the variation which they present among themselves. In short, the similarity of the Mexican and Wyoming shells is surprisingly close, considering their wide geographical separation.

These facts, together with others already published, show more and more clearly the integrity of the molluscan fauna of the great ancient intra-continental sea in which the Laramie Group was deposited, and its separateness from the faunæ of all other North American groups of strata.

ART. XXIV.—*The Sphingidæ of North America;* by A. R. GROTE, A.M.

THE readers of this Journal may be interested in a brief review of the present knowledge of our *Sphingidæ*, and the contrast it offers with that contained in Dr. Harris's article, published in this Journal, II, vol. xxxvi, p. 282, *et seq.* Dr. Harris's paper bears the title of "Descriptive Catalogue of the North American Insects belonging to the Linnæan genus Sphinx," and certain forms were included by him which now are placed in distinct families and are not considered in the present paper.

Dr. Harris divided the "Sphinges legitimæ" into three families of equal value, viz: *Sphingiadæ*, *Macroglossiadæ* and *Ægeriadæ*. The last is not considered a distinct family in the Latreillean sense. It varies by the larval habit and structure, no less than in peculiarities of the imago, from all known families of Lepidoptera, whereas the two former of Dr. Harris's "Families," possess only comparative distinctions of subordinate value, such as the absence or presence of the caudal tuftings, to authorize their separation. The larval structure is essentially similar, as all the details of the immature stages.

The issue of Dr. Harris's article was followed, in 1859, by an elaborate monographic paper from the pen of the late Dr. Clemens, in which the structure of the group was fully discussed, and the family term *Sphingidæ* used in the sense in which it is held in the present paper. In 1865 appeared "A Synonymical Catalogue of North American Sphingidæ," by the writer and the late Mr. Coleman T. Robinson, in which the synonymy of the species, which had been copied by Clemens from Walker's British Museum Lists, was again originally investigated, with the effect of establishing the synonymy since adopted in this country, and, so far as our fauna is concerned, abroad. Subsequently, the writer has revised the genera in various papers, and they are now brought into comparative accordance with the results reached more recently by Mr. Butler in his revision of the *Sphingidæ* from all parts of the world as far as known. In the present paper the writer endeavors to show the probable origin of the various Sphingid genera found in our territory, from a study of their structure and the representation of the family elsewhere.

In Dr. Harris's communication to the pages of this Journal, nine genera are adopted and thirty-four species, one being extra-limital. Under the term "North America," the territory north of Mexico and the West Indian Islands, is here, following the leading zoologists, intended. In my late "New Check

List," I have recorded thirty-four genera and ninety-one species from this territory.

Of the species enumerated by Dr. Harris, the names of six, *Quadricornis* (=*Amyntor* Hubn.), *Carolina* (=*Celeus* Hubn.), *Cinerea* (=*Chersis* Hubn.), *Sordida* (=*Eremitus* Hubn.), *Satellitia* (=*Pandorus* Hubn.), *Pelasgus* (=*Thysbe* Fabr.), were incorrectly given. Dr. Harris was unacquainted with Hubner's illustrations and works, and these corrections were found necessary by ourselves and other later students. Although the generic numbers have been subsequently increased, many of these names now used have become necessary from the discovery of new structural forms of the group within our territory, for instance, the genera *Euproserpinus* Grote and Robinson, and *Arctonotus* Boisd. Others have become necessary through the comparison of our *Sphingidæ* with those of other countries. For instance, so long as we only consider our own fauna, we might include our species of *Smerinthinæ* under one generic title simply as a matter of convenience. But when we find that our *Smerinthi* have varying affinities in other countries, and again contain certain exclusively American forms, we shall lose sight of their ultimate peculiarities of structure and their probable origin, by leaving them thrown loosely together under a common designation, which conceals valuable facts from public display.

Among the rich discoveries of the lamented elder Agassiz is the existence, in the southern portion of the Floridian peninsula, of a colony of animals belonging to the West Indian fauna. The collection of Floridian *Sphingidæ* in the Museum of Comparative Zoology has been examined by me, with the result of adding to our lists *Amphonyx Antæas*, *Dilophonota Alope* (=*Edwardsii* Butl.), etc., while collections from Indian River in Mr. B. Neumœgen's cabinet have added *Cautethia Grotei* Edw., from Florida and Cuba, to our fauna. As we entertain within our boundaries an Arctic colony on Mount Washington (my paper on which appeared in this Journal in 1875), we have a tropical settlement in Florida, the development of our knowledge of which is of the greatest zoological importance. At present the conclusions to which I have arrived from a study of the *Lepidoptera* may be stated here as follows: the stronger-winged and more migratory forms, *Sphingidæ* and *Noctuidæ*, are generally the same species as the West Indian; the weaker-winged forms, *Zygænidæ*, *Bombyces*, *Geometridæ*, show representative or distinct species. In the former case there is probably a constant infusion of new blood borne in on swift and nervous wings. In the latter, we may often have to consider that we are dealing with the descendants of forms established in the peninsula at the time of its separation

from existing islands in the Gulf of Mexico. Their wide wings break in the task of carrying their heavy bodies on the south winds, while their habit is sluggish and unambitious of long and steady flights. The length of "tongue" and custom of flower-feeding distinguishes the first of these two classes. The last contains species that taste but little honey and are shorter-lived in the perfect state. Indeed, some have the maxillæ quite short and weak and even aborted.

To return to the test of generic characters offered by our *Sphingidæ* on a comparison with allied forms elsewhere, I show that our *Smerinthi* belong to several distinct genera, based on the form of the wing, the nervulation, the peculiarities of the vestiture, shape of body, structure of palpi and antennæ. The genus *Smerinthus* of Latreille is based on the *Ocellatus* L. of Europe. We have one species from California, *Ophthalmicus* Bd., and its varietal form *Pallidulus* Edw., which is strictly congeneric with *Ocellatus*. Usually a case like this is used to show the greater resemblance of the western fauna with that of Europe as compared with our eastern animals. But I believe we should regard it as showing that both are descended from a Tertiary circumpolar fauna, and that the occasion for the differing distribution is due to the topography of the country. The migration which took place in the latter part of the Tertiary seems to be proved by the present location of such forms. The Rocky Mountains played an important part in the distribution of animals, dividing races, and limiting the range of species. In other papers I have attempted to show that the peopling of the continent came by way of the north and Behring's Straits during a former geological epoch. When we turn to our insects I have recently discovered, in New Mexican collections made by Professor Snow, a form, *Copimamestra Occidenta* Grote, living remote from the sea-boards, while representing the European *Copimamestra Brassicæ* in ornamentation, hairy eyes, tibial claw and thoracic structure. Such a discovery (just published by me) cannot be explained otherwise than that both were connected in former ages, and are remnants of a circumpolar fauna. The genus has not crossed the tropics. In the *Smerinthi* we have further the genus *Triptogon*, with two species, *Modesta* of Harris and *Occidentalis* of Edwards. An apparent larval modification of *Modesta* has been announced as a distinct species from Louisiana under the name of *Cablei*. But though the larva feeds on water plants and exhibits a difference in possessing dorsal tubercles or "horns," the drawing of the moth is so like our northern *Modesta*, which was unknown to the discoverer of *Cablei*, that there must be some mistake about it, and the species stands on a bad footing. But this genus is numerously represented by closely allied forms in Asia, as we

learn from Butler's treatise, and it is clear that ours must have originally come to us from the common ancestry of all the present species. For the American type of *Excæcatus*, I have retained Hubner's term *Paonias*. For the equally American type *Astylus*, I have proposed the necessarily new term *Calysymbolus*. Our remaining Smerinthoid form, *Juglandis*, differs more strongly than almost any structural form in the group from its allies. Confounded by Hubner with the rest of the gray *Smerinthi* under *Polyptichus*, it has been properly made the type of the genus *Cressonia* Grote and Robinson. The prejudice against the use of Hubner's genera will not find favor with those whose chief concern is the fact which lies beneath the term, and not the name used to show that it is apprehended.

We have, then, three proximate sources for our Sphingid fauna, which to-day consists of ninety-two or three species. That this number will not be greatly exceeded by future discoveries may be safely assumed. Although local species may await capture in New Mexico, Arizona or Southern California, the comparatively wide range and the muscular activity of these insects preclude the idea that this number will receive accessions as in the past. It has trebled since Harris's time, but the principal additions now seem likely to be from stragglers over our southern borders not yet noted in our lists. Again, in the genus *Hemaris* it may be that one or two names will drop from the list. Until all the facts are known, through a careful breeding of the species, it will not do to be hasty in drawing in any existing names in our comparatively small number of species.

The three elements, from as many sources, in our *Sphingid* fauna, are found first in descendants of a former circumpolar fauna; secondly, in accessions from the tropics, a movement from south to north being still in progress; thirdly, in those genera which have originated within our limits and are peculiarly North American in character. We may thus tabulate the genera of North American *Sphingidæ:*

1. *Descendants from a Circumpolar Fauna.*

Genus.	No. of Species.	Genus.	No. of Species.
Hemaris	14	*Triptogon*	2
Pogocolon	4	*Diludia*	3
Deilephila	2	*Sphinx*	15
Ampelophaga	1	*Hyloicus*	3
Smerinthus	1		
Eusmerinthus	2	10 genera and	47 species.

2. *Accessions from the Tropics.*

Genera.	No. of Species.	Genera.	No. of Species.
Ællopos	2	*Chœrocampa*	2
Cautethia	1	*Amphonyx*	1
Amphion	1	*Phlegothontius*	4
Enyo	2	*Dilophonota*	6
Philampelus	4		—
Argeus	1	11 genera and	26 species.
Pachylia	2		

3. *Genera of North American origin peculiar to this Continent.*

Genera.	No. of Species.	Genera.	No. of Species.
Lepisesia	1	*Cressonia*	1
Euproserpinus	1	*Ceratomia*	3
Thyreus	1	*Daremma*	3
Deidamia	1	*Dolba*	1
Arctonotus	1	*Ellema*	3
Everyx	2	*Exedrium*	1
Paonias	1		—
Calasymbolus	2	14 genera and	20 species.

In these tables there is no separate account taken of the species of *Sphinx*. This genus cannot be satisfactorily split up, as I am sure that the different divisions proposed (e. g. *Lintneria*) have not sufficient or any real characters to sustain them. It is not as with *Smerinthus* where objective differences underlie all of the divisions. But many of the species fall evidently under either the second or third group. While *drupiferarum* is evidently allied to the European species, *Gordius* and allies are probably of southern extraction, and in *Elsa* and *Dollii* we have probably forms of American origin. The genus belongs to an older period of separation. The position of *Diludia* and *Daremma* is not assured, but I prefer the present to my first arrangement of the genera. We have neither *Macroglossa* nor *Acherontia;* the decisive element in our fauna does not come from the Old World.

But on the whole the tables are probably approximately accurate, sufficiently so as to draw attention to the class of facts which a study of this interesting group offers, and in presenting them, in connection with a resumé of our knowledge as to the number of kinds of North American *Sphingidæ*, the writer asks the indulgence of the reader. They have been the result of a long-continued study of this group of insects, and are put forth to invite the attention of entomologists to the recording of a class of facts which are of value in the large question of the distribution of life upon this globe, and which relieves entomology from the popular charge of being the cause of a frivolous waste of time upon insignificant objects. The writer also desired to trace subsequent progress in our knowledge of a subject which the late Dr. Harris had offered in this Journal nearly half a century ago.

Art. XXV.—"*Rotational Coefficients*" *of various Metals;* by Edwin H. Hall.

The experiments described below were made at the Laboratory of Harvard College during the summer of 1882, and most of the results obtained were given at the Montreal meeting of the American Association.

At the York meeting of the British Association, Sept., 1881, I gave a list of certain metals with an approximate value of the "rotational coefficient"* for each as determined by my experiments. This list was published in the report of the Association. Several of these metals had, however, been examined in an extremely inaccurate manner, as was stated at the time, and the numbers assigned them were marked as doubtful. Thus a part of the list ran:

Name of Metal.	Rotational Coefficient, Arbitrary Scale.
Zinc	+15· ?
Aluminium	−50· ?
Magnesium	−50· ?
Copper	−10· ?
Brass	− 1·3 ?
Lead	No effect discovered.

Repeating, still in a hasty and rough manner, but more carefully than before, the experiments with all these metals except magnesium, and using indeed the same pieces of metal as before, I found:

Name of Metal.	Rotational Coefficient.
Zinc	+10·5
Aluminium	−37·
Copper	− 6·5
Brass	− 1·4
Lead	No effect discovered.

It will be observed that the value obtained for brass, which is small, is but little changed, but those for zinc, aluminium and copper have each been reduced about 25 or 30 per cent. We may perhaps by analogy, without actual determination, write:

Magnesium	−35

All these values may still be subject to errors of 10 or 20 per cent, but will nevertheless serve present purposes tolerably well, if substituted for those given in the list previously published. Such a list, though rough, may be compared with other

* Phil. Mag., Sept., 1881, p. 162.

lists in which the same metals are arranged relatively to various physical properties, and any analogies thus suggested may be tested farther by more accurate and detailed investigations. In fact, to go no farther than the above table itself, the fact that the small rotational coefficient in brass lies between the positive coefficient in zinc and the negative in copper suggests the advisability of a careful study of the transverse effect in alloys.

In the Phil. Mag. for Sept., 1881, I stated that the transverse current obtained with a nickel strip is much increased, other conditions remaining unchanged, by rise of temperature. It was a question of much interest whether the transverse current in the non-magnetic metals would prove to be affected in a similar manner. It might be found indeed that the transverse current would increase at the same rate as the electrical resistance, in which case it would appear that the transverse effect depends upon the rate of fall of electric potential along the strip of metal rather than upon the strength of the direct current.

Accordingly, from a sheet of No. 2 gold foil,* the thinnest foil used by dentists, a piece was cut in the form of a Greek cross. The extremity of each arm of this cross was soldered to a disk of brass. The four brass disks were screwed to a plate of hard rubber in such a manner as to extend the arms of the cross, which was then fastened to the rubber plate by means of melted resin run between. Wires soldered to the disks served for the connections.

The very considerable difficulty of soldering so thin a strip of gold and then fastening it upon the plate, has heretofore prevented my employing this method of making connections with that metal, screw clamps being used instead. The practical advantage of soldered connections is of course considerable, though not so great as might at first appear. Resin, although very brittle, was used as a cement, for the reason that within the limits of temperature to be employed it is more rigid than any other cement I could hit upon, and therefore less liable to allow the gold strip to become distorted or strained by the stress it is subjected to while carrying a current across a powerful magnetic field.

In order to control the temperature of the gold strip, it was placed in a narrow tank between the poles of the electro-magnet, and water was made to flow slowly through the tank, from bottom to top, during the experiment. The lowest temperature used was about 2° C., the highest about 30° C., as will be seen below.

Aug. 2 the following results were obtained in the order given:

* "Standard," R. S. Williams & Co.

Gold.

Temperature.	Numbers proportional to transverse effect.
30°·2 C.	1738
2°·2	1703
2°·6	1748
30°·0	1746

No attempt was made to determine the absolute magnitude of the rotational coefficient in this specimen of gold, so that the numbers given in the second column must not be used for comparison with numbers elsewhere given as proportional to the rotational coefficient in gold or other metals.

The magnetic field in these experiments had an intensity of about 1900 C. G. S. The primary current was not measured in absolute units. It was such as a Bunsen cell yields in a circuit of a few ohms resistance.

From the above table we get:

Temperature.		Numbers proportional to transverse effect.	
30°·2 30°·0	30°·1	1738 1746	1742
2°·2 2°·6	2°·4	1703 1748	1726

The mean of the numbers at low temperature is therefore less than the mean for the higher temperature by rather less than one per cent. The particularly small number 1703, which causes this result, was obtained from a rather bad series of observations, and is probably entitled to less weight than the others. In fact, though the numbers as they stand seem to indicate a decrease of about one per cent in the value of the transverse effect for a fall of about 30° C., I think it better to say that we have here detected no certain effect of fall of temperature.

It is evident from these experiments that if the value of the transverse effect in gold varies at all with change of temperature, it varies far less than the electrical conductivity. We must conclude, therefore, that this effect depends rather upon the magnitude of the current through the gold than upon the fall of potential per unit of length. This conclusion was long ago* reached, but doubt had been cast upon its correctness by the experiments upon nickel above-mentioned.

Turning again to the magnetic metals and taking a strip of thin iron, experiments were made similar to those with gold as above described. The results obtained, though by no means so accurate as those obtained with gold, show that in iron the rotational coefficient is very strongly affected by change of

* This Journal, Sept., 1880.

temperature, the effect being an increase of perhaps two-thirds of one per cent for a rise of 1° C. Possibly a comparison of the effect of change of temperature upon the magnetic permeability of iron, nickel and cobalt, with the effect of the same change upon the rotational coefficient, will be of value when both effects shall have been more fully studied.

Leaving the matter of effect of change of temperature and referring again to the article on nickel and cobalt (Phil. Mag., Sept., 1881), we see that the rotational coefficient in nickel decreases as we increase the strength of the magnetic field; i. e. the rotational *effect*, other things being equal, increases less rapidly than the intensity of the magnetic field.

Experiments were made for the purpose of determining whether a similar relation would hold in iron. The iron was tested in magnetic fields varying in intensity from about 1000 to about 7500 in absolute C. G. S. units. Judging from the behavior of the strip of nickel previously examined in this manner, the R. C. of that metal would be about 20 per cent greater in a field of intensity 1000 than in one of intensity 7500. For certain reasons I do not feel perfect confidence in the numerical results obtained with iron, and do not consider them worth publishing. To myself, however, they make it seem probable that the rotational coefficient in iron is *less* in a field of intensity 1000 than in a field of intensity 7500, and I expect to prove this when I am able to take up the matter again. Cobalt should of course be examined in the same way, nor must it be forgotten that it is by no means proved, as yet, that the non-magnetic metals will show a constant rotational coefficient when tested between wide limits of magnetic force.

The object of another experiment was to determine, if possible, whether any part of the rotational effect could be made permanent under favorable conditions. For this purpose a piece of clock spring was taken, tempered very hard, and then reduced by action of nitric acid to a thickness of about ·06^{mm}. This piece of steel was firmly imbedded upon a plate of glass in a layer of cement made of melted beeswax and resin. This plate, with the usual electrical connections, and with a current flowing through it, was placed in the usual position between the poles of the electro-magnet; the magnet current was turned on, then off, and the plate removed from between the poles in order to avoid the action of the very considerable residual magnetism of the electro-magnet. A reading of the Thomson galvanometer in the transverse circuit was now made, then the plate was replaced between the poles and the current turned on again but in the opposite direction. The magnet current being again interrupted, the plate was again removed from the field and another reading of the Thomson galvanometer was made.

The two readings differed by several centimeters on the galvanometer scale. The experiment was repeated and always with a like result.

There was no room for doubt that the direction of the equipotential lines in the steel was permanently changed by the action of the magnet. This change was in the same direction as the temporary change produced by the magnet's action, and perhaps equal to 2 per cent of the temporary change.

This result was of course not unexpected. The hardened steel must have become permanently magnetized transversely, and this magnetization should produce an effect similar to that of temporary magnetization. The experiment is of interest, however, as indicating that the rotational effect is not due to the mere mechanical stress to which the metal is subjected in the magnetic field, for though no one has ever pointed out how any such stress could produce the effect observed, many have no doubt questioned whether it might not after all be due in some obscure way to such stress.

It may be stated incidentally that the transverse effect appears to be much greater in steel of blue temper than in soft iron, and again much greater in steel of very hard temper than in steel of blue temper. If we call the effect in soft iron 1, the effect in blue steel is perhaps 2, and that in very hard steel 4.

ART. XXVI.—*Recent Exploration of the volcanic Phenomena of the Hawaiian Islands;* by Captain C. E. DUTTON. (From a letter to J. D. Dana, dated Washington, D. C., Feb. 8, 1883.)

RETURNING from my long visit to the Hawaiian Islands, I feel that I owe it to you to make some return for the kind interest you took in my journey and for the valuable suggestions you made me prior to my departure. I therefore avail myself of a convenient opportunity to tell you briefly some of the matters which most particularly interested me.

After making such purchases as were thought necessary for my journey at Honolulu, I took the inter-island steamer for the southern part of Hawaii. I did not go to Hilo at first, as travelers generally do, for after making inquiry, I came to the conclusion that the southern part of the island would be a much more advantageous position from which to begin the study of Mauna Loa and Kilauea. The Hilo side of the island is very rainy. The field geologist quickly gets accustomed to every inconvenience and discomfort of travel except one, and that is mud: and the more he has to do with mud the more he hates it. The southern district of the island, Kau, is almost

always dry and the traveling good enough. I fitted out a pack-train with six packs, in regular Rocky Mountain style, and my first journey, of course, was to Kilauea. It is far pleasanter to approach the volcano from the Kau side than from the Hilo side, and the journey was full of interest. My first visit to Kilauea lasted ten days, during which I explored the great pit thoroughly and also the country round about. If I can rightly estimate the accounts of observers who saw Kilauea forty years or more ago, I should infer that the total amount of volcanic energy now manifested there has very considerably diminished. There is difficulty, however, in forming an estimate of how much allowance should be made for the enthusiasm and excited imaginations of travelers who for the first time, and generally the only time, have beheld this wonderful spectacle. The great inner pit, which was first described by Ellis in his Polynesian Researches, in 1823, and also by yourself, in 1841, has been completely filled up. The great outer cavity also has, I infer, become notably shallower, having been partially filled by innumerable overflows of lava. The inner cavity, which once held a burning lake, is now represented by two lakes, whose united surfaces have, I should judge, an extent which is but a small fraction of the surface of the old lake of forty or fifty years ago. These two lakes are both situated with their surfaces at levels higher than the mean level of the main floor of the pit. I infer too that they are much more languid and sluggish in their action than the lake which you saw.

The height of the walls surrounding the pit varies from 320 to 740 feet. There is abundant evidence that the floor of the pit sinks down more or less after every eruption within it, but presumably not to so great an extent as to compensate the building up of the floor after the successive out-pours of lava, so that, on the whole, the pit is probably growing shallower.

I watched, with the deepest interest, the action of the lava in the lakes. The most accessible one is now called the New Lake. It undergoes a series of regular changes within a period of about two hours. When we reach the brink of it we generally find it frozen over and quite black and still, except at the edges, where we perceive a rim of fire. We observe also at many places upon the edges a little sputtering and blowing out of lava and hear a dull simmering sound. At length a piece of the black lava upon the surface cracks, turns down its edge and sinks, disclosing a patch of livid fire. Soon after in some other part of the lake, at the edge, another piece breaks and goes down. This becomes more and more frequent until at last a hundred cracks suddenly shoot through the entire surface, and, with a grand commotion, numberless

fragments of the frozen surface plunge downward, leaving the whole one glowing mass of lava. For a few minutes the spectacle is very grand, but it does not last long. The surface quickly darkens and freezes over again, becoming black as before and in this condition it remains for an hour or two. The period between break-ups is not regular, being as short as forty minutes and as long as two hours and a quarter.

The explanation of the phenomenon is, I think, not difficult. When the lava first passes from the liquid to the solid condition, while its temperature is still near the melting point, but below it, its density is less than that of the lava below. As the crust thickens and the surface becomes cooler, its density becomes greater than that of the lava below, and its position then becomes unstable. A slight disturbance produces a rupture, and the sinking of one fragment is quickly followed by that of the others.

It has been the custom to speak of Kilauea as being situated upon the flanks of Mauna Loa and to regard it as a mere appendage of that mountain. But it presents itself to me as a distinct volcano having no more connection with Mauna Loa than Mauna Kea has. Into the discussion of this I cannot now enter.

From Kilauea I went to Mauna Loa. My first objective point was the source of the last great eruption of 1880–81. It is reached with difficulty on account of the roughness of the clinker fields, or *aa*, as it is termed in the islands. The vents are situated from twelve to eighteen hundred feet below the summit, upon the northeastern spur. Three distinct streams flowed from as many vents, one flowing northward to the base of Mauna Kea, a second flowing southward into Kau, and the third, and by far the largest, flowing first northward then deflecting eastward until it came within half a mile of Hilo. This latter stream was about fifty miles in length and varied in width from half a mile to two miles. The appearances presented at this point I shall describe at a future time. It may be sufficient to state here that a series of parallel fissures pointing from the summit toward the base of the mountain gave issue to the lavas. No cone was built, and there is no accumulation whatsoever of fragmental eruptive products.

I was deeply impressed with the colossal character of the eruptions of Mauna Loa. Of the eruptions which bear historic date that of 1855 appears to have been the grandest. It would have almost built Vesuvius. The accounts given to me by many eye witnesses of these eruptions recite observations which strike me as most extraordinary, though I cannot for a moment question the general truthfulness of these ac-

counts attested by so many intelligent and credible witnesses. The general version is that they break out suddenly and without warning, and that the lava spouts upward in enormous fountains to a great altitude, which the various observers estimate all the way from 500 to 1000 feet. How much of this may be attributed to incandescent steam and how much to optical illusion of one kind or another it is impossible to say. But I cannot doubt the general testimony that these vast lava fountains do spout upward to a very considerable height, and that the fires which are actually seen are mostly lavas. I think there is substantial evidence of this in the appearances presented at the sources of the great eruptions of 1855, '59 and '68. Dr. Coan visited the source of the eruption of 1855 while it was still active; and about three months before his death I had the privilege of inquiring of him very particularly about this matter, and his account substantiates the general testimony.

One of the most striking features of Mauna Loa is the almost total absence of cinder cones. There are a few small piles of fragmental material here and there, but they are mere apologies for cinder cones and are very aberrant in their modes of aggregation and in the character of component materials. Considering the portentous nature of these monstrous outbreaks, it is wonderful how little disturbance attends them. No earthquakes, no rending and shaking of the mountain nor roar of escaping vapors, no vast clouds of steam, but simply a huge river of fiery lava welling forth like water from a fountain and flowing swiftly on its course down the mountain side. So far as I have ever heard, this quiet character of the eruptions, the absence of fragmental products, and the insignificant amount of elastic force exerted by escaping vapors are without a parallel.

All of the great eruptions of Mauna Loa come from fissures which point from the summit of the mountain directly down its slopes.

I visited the great pit at the summit of Mauna Loa twice from two different lines of approach. It is very nearly equal in its horizontal extent to Kilauea, but it is much deeper, being about a thousand feet in depth, and is a much more impressive spectacle. It was absolutely still, without a trace of igneous action at the time of my visit. Before the last great eruption it was in a state of intense activity, spouting out lava in jets which attained a height of seven or eight hundred feet, and the igneous phenomena were, judging from all accounts, far more impressive than those of Kilauea. The glare of its fires was seen a few days before the last eruption; but it would seem that as soon as the last eruption began, the vents

at the summit immediately sealed up, being tapped, I presume, by the outbreaks which occurred at a considerably lower level.

The lavas of both Kilauea and Mauna Loa seem to me to be of an abnormal type. The analyses are not yet made and I can therefore give only their superficial character. They have the appearance of being extremely basic, decidedly more so than normal basalts. I cannot help thinking that they may be fairly relegated to what Judd describes as ultra basalts. Most of the lavas of Mauna Loa contain excessive quantities of olivine, many specimens being at least half composed of that mineral. The lavas of Kilauea, on the other hand, whether in the pit itself or in the country round about, seldom show much olivine. But the eruption of 1840, which belongs physically to the Kilauea group, is highly olivinitic, while the last eruption of Mauna Loa shows little or no olivine. I am led to suspect that the ultimate analyses of the two lavas, whether olivinitic or not, will show but little difference. In other words, I suspect that in some cases the olivine was crystallized in the lava before eruption, while in others it was not, the magma being very nearly identical in both cases.

I spent a great deal of time in the study of Mauna Kea. This volcano contrasts strongly in its aspect with Mauna Loa. Its lavas are apparently more nearly normal basalts and show a somewhat wider range of variety. The most striking difference in the two mountains is the absence of fragmental products upon Mauna Loa and their great abundance on Mauna Kea. The latter mountain is covered all over with magnificent cinder cones of large size and beautiful proportions, which are, by far, the most striking features of its mass. Many superb cinder cones are scattered thickly around its base and over its great flanks, and a large cluster of them forms its summit. The activity of Mauna Kea has probably been extinct for a very considerable period of time. When we first look upon its cinder cones in a perfect state of preservation, the first impression is in favor of great recency in its activity, but a more careful study of the surroundings leads to a modification of this view. Upon the windward side of the mountain the ravages of time are very apparent and quite extensive. Upon the leeward side they are far less extensive, but are by no means wanting. During the past few years my attention has frequently been called to the very great inequalities of effects produced upon the same mass by varying degrees of energy in the agencies of degradation. Nowhere does it come out more clearly than in these islands. The windward sides in most cases have been devastated to an astonishing degree, so much so that I sometimes shrink from the task of trying to convince anybody of the reality which I am sure of. But on the lee-

ward sides, which have undoubtedly been exposed for an equal period of time, the degradation is but a small fraction of what appears upon the windward sides.

The cause of the difference in the forms of Mauna Loa and Mauna Kea is very apparent; the former being built up entirely of fluent lavas, without fragmental products; and the lava streams being of great magnitude, the ejected material has diffused itself over a very wide extent of country and flowed many miles away from the principal focus of eruption. The mountain, therefore, is abnormally flat in its profile. In Mauna Kea, on the other hand, so large a proportion of the ejecta being in a fragmental form, they are piled up around the places where they were thrown out. The mass of Mauna Kea is many times smaller than that of Mauna Loa; but the top of its summit platform is only six or seven hundred feet lower than that of Mauna Loa, while the cinder cones upon the summit carry its apex about two hundred feet higher than the summit of Mauna Loa.

On all the slopes of Mauna Loa there is nowhere to be found anything like a ravine. Nor is there a single living stream, however small. And yet on all sides the precipitation is very great, but the water sinks as rapidly as it falls. The lava is highly vesicular and much broken, never compact except in bands here and there, at the bases of the larger flows. Every lava stream gives rise to long pipes or tunnels and there are literally thousands of them, some of which are several miles in length. In truth, these long caverns must form an appreciable portion of the entire volume of the mountain. Remembering also the very vesicular character of the lava, it seems plain that while the absolute density of the materials is very high, the specific gravity of the mass as a whole is by no means so.

It appears to be a general fact throughout the islands that erosion does not take hold of these volcanic piles to any appreciable extent during their activity, and after they become extinct a long period must still elapse before surface erosion other than chemical weathering can begin. The cutting of ravines is impossible without running water, and the water cannot collect in streams until the cracks and pores of the lava are silted up. Of course, this takes place more quickly upon the windward than upon the leeward sides. These facts are abundantly illustrated on every island in the group.

I also visited Hualalai, which has an altitude of about 8,600 feet. It seems to be intermediate as regards the character of its lavas and many of its eruptions between Mauna Kea and Mauna Loa; being more basic than the former, less so than the latter. It has many cinder cones upon it, especially at the

summit, some of which resemble those of Mauna Kea, while others have the abortive, abnormal and dwarfed character of the very few which occur upon Mauna Loa. This volcano it is well known has been active in the early part of the present century. From 1801 to 1811 there were three distinct eruptions, separated by intervals of a very few years, but all of them were small. One of them, as nearly as can be made out, must have occurred about the year 1801, the second in 1805, and the last in 1810 or 1811.

Kohala Mountain, at the north end of the island, is about 5400 feet in height, and its activity, no doubt, ceased at an earlier period than that of Mauna Kea. Its lavas are largely normal basalts, much of it approaching andesite in character. It appears to be notably less basic on the whole than the lavas of Mauna Kea. It has many cinder cones, some of them perfectly well preserved, others showing conspicuous traces of decay.

My visit to Maui, though briefer than that to Hawaii, was very interesting. The great volcano Haleakala is about 10,400 feet high. The great "crater" (so-called) at the summit possesses a grandeur and impressiveness which have not been overrated by travelers who have heretofore described it. The form of this summit depression is certainly most extraordinary and not easy to account for. It is impossible, however, to describe this mountain briefly, and I shall not here attempt to do so. It is wholly basaltic and in its general characteristics a pretty close imitation of Mauna Loa. The mountain piles which make up west Maui are much older. They are very much degraded by erosion and literally sawed to pieces by gorges and ravines two thousand to three thousand feet in depth, with precipitous walls. Some of the scenery in these gorges possesses a beauty and grandeur seldom equalled. It is highly peculiar, and so far as I know has its counterpart only in other islands of the Pacific. I found here some lavas which appear to be true andesites, though in the main, the rocks are of a mildly basaltic type.

I also went over the island of Oahu pretty thoroughly. It has many points of interest, of which, perhaps, the most notable are the studies of erosion which it presents. I may make the same remark regarding the island of Kauai. It has frequently been noted that the western islands of the group are the oldest and the antiquity diminishes from northwest to southeast. I consider the conclusion safe, however, only to this extent, that the eruptions in the western islands ceased at an earlier period, though it does not necessarily follow that they began any earlier.

There are abundant evidences of recent elevation in the

islands, the amount of which varies greatly. In a few portions there are marked traces of subsidence, though on the whole the elevating movement has greatly predominated. This subject is too complicated to be discussed here.

It would be impossible for me now to give much idea of the new facts I have learned. I have made no grand discoveries and of course I did not expect to. But I have picked up much knowledge of small details, the value of which no one but a geologist can appreciate; no nuggets but a good deal of fine gold. I think I understand much better than I ever did before the action and behavior of lavas, their modes of accumulation and their methods of flowing. To some extent, no doubt, these observations relate to matters peculiar to the islands, and it would not be safe to consider them typical; but I imagine that their utility will not be less on that account.

One of the most pleasing studies in these islands is the climatology. In truth, there are about as many climates as there are square leagues; yet all of them seem to be reducible to ordinary and well known laws, and when understood form some of the most beautiful examples of the operations of those laws which can well be imagined.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Isomorphism of Mass.*—The laws of isomorphism as enunciated by Mitscherlich in 1819 are as follows: 1st, two bodies are said to be isomorphous when, possessing the same crystalline form, they are capable of crystallizing together in the same crystal; and 2d, two isomorphous bodies possess an analogous chemical constitution. Klein has called attention to the fundamental modification of these laws required by the progress of science, and instances as isomorphous bodies forming exceptions to the above laws (1) the double fluorides of titanium with the double oxyfluorides of columbium and tungsten of Marignac; (2) the quadratic metatungstates and the monoclinic metatungstates of Scheibler, the members of both these groups being isomorphous among themselves, though varying in their water of crystallization; (3) certain silicotungstates of Marignac, the acid salts of barium and calcium being isomorphous with the rhombohedric form of the acid, and the monosodic salt with fourteen molecules of water, with the disodic salt containing eleven; (4) anhydrous sodium sulphate, Na_2SO_4, with sodium chromate, Na_2CrO_4, $(H_2O)_2$, as observed by Wyrouboff; (5) the double chlorides $(KCl)_3InCl_3(H_2O)_{1½}$ and $(KCl)_3TlCl_3(H_2O)_2$, observed by Fock; and (6) the borotungstates, noted by Klein himself. These are: a borotungstic acid $(WO_3)_9B_2O_3(H_2O)_2$, $(H_2O)_{22}$, with

the silicotungstic acid of Marignac $(WO_3)_{12}SiO_2(H_2O)_4$, $(H_2O)_{29}$, and also with the monosodic borotungstate $(WO_3)_9B_2O_3Na_2OH_2O$, $(H_2O)_{22}$; a barium borotungstate $(WO_3)_9B_2O_3(BaO)_2$, $(H_2O)_{18}$, with the barium metatungstate $(WO_3)_4BaO$, $(H_2O)_9$ of Scheibler; a diammonium borotungstate $(WO_3)_9B_2O_3(NH_4O)_2(H_2O)_2$, $(H_2O)_{19}$ with an ammonium metatungstate described by Marignac. The isomorphism in these cases is complete; so much so that, for example, a crystal of barium metatungstate will cause a supersaturated solution of di-barium borotungstate to crystallize at once. Accepting a suggestion made by Marignac some time ago, that two compounds containing a common element or group of elements constituting the greater part of them by weight, may be isomorphous even if they have not a similar atomic constitution, Klein formulates the laws of isomorphism thus: 1st. Two bodies are called isomorphous when they have the same crystalline form and are capable of crystallizing in all, or in very variable proportions in the same crystal. 2d. Two isomorphous bodies have either an analogous chemical composition (constitutional isomorphism), or are formed in great part of a group of elements which are common or have an analogous function (mass isomorphism). The exceptional cases above given are all included under the law of isomorphism of mass.—*Bull. Soc. Ch.*, II, xxxix, 10, Jan., 1883. G. F. B.

2. *On a case of Physical Isomerism.*—Lellmann has observed a marked case of physical isomerism. Dibenzoyldiamidodibromdiphenyl, prepared by benzoylizing Fittig's diamidodibromdiphenyl, crystallizes from hot alcohol in fine colorless needles which fuse in capillary tubes at 195°. When the mass has become liquid, if the tube be suddenly removed from the bath and quickly cooled, the contents solidify to a transparent vitreous mass which, placed in a cold bath and gradually heated, fuses at 99°. If the heat be continued, the resulting liquid solidifies again between 125° and 130° to an opake crystalline mass, which again fuses at 195° as before. The changes are better observed by heating the substance as crystallized from alcohol on a watch glass over a flame and then cooling. The glassy mass is pulverized and placed in a fine tube, where its liquefaction is more readily observed on heating. It would appear that the sudden cooling from 195° to the ordinary temperature, causes it to assume another physical state; because if allowed to cool slowly no such change takes place.—*Ber. Berl. Chem. Ges.*, xv, 2835, Dec., 1882. G. F. B.

3. *On Nitrogen Selenide.*—Verneuil has examined the selenide of nitrogen discovered by Wöhler in 1859. The process of preparation used by the discoverer was to saturate selenium perchloride with ammonia gas. But the author finds that Fordos & Gelis's process for nitrogen sulphide gives better results. Ten grams of the perchloride are mixed with a few drops of carbon disulphide, and the paste thus made is suspended in a liter of CS_2 in which it is almost insoluble. Into this liquid a current of dry ammonia gas is passed. Flocks of ammonium chloride are precipitated and the liquid passes from a rose tint to a dark cochineal-

red color. Finally the red color disappears and brown flocks are thrown down. The current of gas is continued until these flocks become of a clear orange tint. The liquid is filtered, the flocks washed with CS_2 and dried. On removing the NH_4Cl with water, washing again with CS_2 and drying, the nitrogen selenide is obtained pure, in amount equal to 80 per cent of the theoretical yield. It forms an amorphous orange powder, insoluble in all solvents, having the formula Se_2N_2. When dry it detonates violently by a shock, being as easily exploded as mercury fulminate, less easily than nitrogen iodide. Potassium hydrate and hydrogen chloride decompose it, producing selenite of potassium or ammonium.—*Bull. Soc. Ch.*, II, xxxviii, 548, Dec., 1882. G. F. B.

4. *On the Preparation of Carbonous oxide.*—Noack, observing, in his reduction experiments with triphenylphosphites, the action of zinc dust upon carbon dioxide, has proposed this as a method of preparing carbonous oxide. For this purpose a somewhat wide combustion tube is used, filled with zinc dust for its whole length, a passage being left for the gas. This tube is heated to 400° in a furnace, and CO_2 is passed into it from an evolution flask, a wash-bottle containing Na_2CO_3 being inserted between it and the tube, and a second one containing potash solution at the farther end. With 200 grams of zinc dust, 20 liters CO can be obtained in a short time. Since the volumes of CO_2 and of CO are equal, the quantity of the latter obtained should equal the former; but in practice there is some loss, thirteen liters CO_2 yielding only eleven liters of CO. The gas is exceptionally pure. —*Ber. Berl. Chem. Ges.*, xvi, 75, Jan., 1883. G. F. B.

5. *On Molecular Compounds of Benzene and Naphthalene with Antimonious chloride.*—Smith and Davis have observed that on melting a mixture of three parts of antimonious chloride and two of naphthalene, a beautiful crystallization commences on cooling, perfectly symmetrical monoclinic plates being produced. With some difficulty enough of these crystals were removed to be examined. They are very deliquescent and must be placed at once in a stoppered bottle. On analysis they gave numbers corresponding to the formula $(SbCl_3)_2\ (C_{10}H_8)_3$. If the $SbCl_3$ be dissolved in benzene, three parts of the former to four of the latter, and the corked flask be set aside for a few days, large well-defined monoclinic plates thinner and less regular than those of the naphthalene compound are produced. They are colorless and transparent, and are very deliquescent. On analysis they gave numbers agreeing with the formula $(SbCl_3)_2\ (C_6H_6)_2$.—*J. Chem. Soc.*, xli, 411, Dec., 1882. G. F. B.

6. *On Acetoxims, a new class of Organic bodies.*—Janny, under v. Meyer's direction, has succeeded in producing a new class of organic bodies by the action of hydroxylamine upon various ketones. These new bodies he calls acetoxims, and an acetoxim he defines to be a body containing the group CNOH = (combined on both sides with carbon. If hydrogen saturates on one side) a body is formed to which he gives the name aldoxim. The

simplest acetoxim is dimethyl-acetoxim, $CH_3-CNOH-CH_3$, or acetoxim proper; analogous to dimethyl-ketone or acetone. It is produced by the action of hydroxylamine upon acetone in the cold in aqueous solution. It is easily soluble in water, alcohol and ether, fuses at 59°–60° and boils at 134·8°. Ethyl-methyl-acetoxim, methyl-pseudobutyl-acetoxim, methyl-phenyl-acetoxim, and diphenyl-acetoxim are described in the original paper.

PETRACZEK has studied the aldoxims in the same laboratory. He describes ethyl-aldoxim C_2H_3NO or $CH_3-CNOH-H$, propyl-aldoxim C_3H_7NO, and benzyl-aldoxim C_7H_7NO. They are formed by the action of hydroxylamine upon the respective aldehydes.—*Ber. Berl. Chem. Ges.*, xv, 2778, 2783, Dec., 1882. G. F. B.

7. *On the Synthesis of Uric acid.*—The synthetic production of uric acid has been accomplished by HORBACZEWSKI. Pure, finely pulverized glycocoll was mixed with ten times its weight of pure urea and heated quickly to 200° or 230° in a metallic bath being kept there until the colorless liquid became a yellow, turbid and pasty. After cooling, the mass was dissolved in dilute KOH, saturated with NH_4Cl and precipitated with a mixture of ammonia-silver solution and magnesia mixture. The precipitate after washing was decomposed with potassium sulphide. The filtrate was saturated with HCl, and concentrated. The crude product by solution in alkali and reprecipitation was purified. A yellowish crystalline powder resulted which possessed all the properties of uric acid. Under the microscope the crystals were plates or rhombic crystals. They reduced copper solution on warming and silver solution in the cold. They dissolved in nitric acid and left on evaporation an onion-red layer becoming purple red with ammonia and violet with potash. They are not soluble in water, alcohol, ether or acids, but soluble in alkalies, and gave the right formula on analysis.—*Ber. Berl. Chem. Ges.*, xv, 2678, Nov., 1882. G. F. B.

8. *The Radiometer.*—Much uncertainty still exists in regard to the phenomena exhibited by the radiometer. These phenomena are complicated by the action of the enclosing vessel, the rarefied medium and the constitution of the vane of the radiometer. ERNST PRINGSHEIM has made a careful study of the influence of the glass-containing vessel, of the enclosed gas and of the constitution of the vane. His apparatus consisted of one vane which was hung by a long bifilar suspension. A little mirror was placed upon the vane, and the movements of the latter were observed by means of a spot of light reflected upon a scale. His experiments lead him to believe that a pressure emanates from the heated side of the vessel, and that this pressure increases with the temperature and is independent of the nature of the material of the vessel. He finds that the absorption by the rarefied medium is extremely small and can be neglected. That the action of the vane is due to the rate of absorption and conduction on its two sides. He considers the theory of currents of the rarefied medium as untenable and believes that the kinetic theory of the radiometer is the

most reasonable one. The influence of the form of the radiometer vanes is due not to the form but to their proximity to the sides of the enclosing vessel. The difficulty of reconciling the various phenomena noticed in radiometers is due to the disturbing causes which result from the various forms of radiometers, and he therefore adopted the simplest instrument, a single vane suspended by a bifilar suspension.—*Ann. der Physik und Chemie*, 1883, No. 1, pp. 1–32. J. T.

9. *Measurement of wave lengths in the Ultra-red portion of the solar spectrum.*—The peculiarity of this measurement by Ernst Pringsheim, resides in the use of a radiometer to measure the heat. The spectrum produced by a Rutherfurd's grating, 17,296 lines to the inch, was examined by keeping all parts of the apparatus stationary except the grating; this was turned upon a vertical axis, and different portions of the spectra were thus thrown upon the radiometer. This radiometer consisted of but one vane, which was suspended by a long bifilar suspension. The radiometer was carefully protected from irregular disturbances by being placed in a suitable enclosure. By means of this apparatus, absorption bands were found from wave length $\lambda = 0{\cdot}0013658^{mm}$ to $\lambda = 0{\cdot}0013908^{mm}$. These results were modified by the absorbing media which were used to separate the spectra of different orders.—*Ann. der Physik und Chemie*, 1883, No. 1, pp. 32–45. J. T.

10. *Phosphorography of the infra-red of the Solar Spectrum.*—Henri Becquerel, in repeating the work of his father, has confirmed the results of the latter and has determined the wave length of a certain band of fine lines denoting the absence of phosphorescence which are analogous to the absorption bands known to exist in the infra-red. A Rutherfurd grating was employed and the spectrum was thrown upon various phosphorescent substances. A table of wave lengths is given, and the presence of maxima and minima of extinction of light peculiar to different phosphorescent substances is noted in the infra-red. Similar extinctions are known to exist in the ultra-violet.—*Comptes Rendus*, Jan. 8, 1883, pp. 121–124. J. T.

11. *Siemens' Theory of Solar Energy.*—M. Faye has pointed out that the centrifugal force at the sun's equator is too feeble in comparison with the force of gravity to enable the sun to play the role of a machine which takes in matter at the poles and throws it off at the equator. Siemens replied to this objection by using as an illustration Newton's tube, which the latter employed to show how the flattening of the earth could be determined. This tube had one of its branches directed along a line from the poles through the center of the earth, and the other from the center to a point upon the equator. Faye replies that Newton's ideal tube was supposed to terminate upon the surface of the globe, while the extremities of Siemens' tube extend indefinitely into space. Thus the sun will communicate his rotation to the entire medium pervading space; and this medium will therefore turn with the

sun—a consequence which M. Faye believes that Dr. Siemens cannot admit.—*Comptes Rendus*, Jan. 8, 1883, p. 79. J. T.

12. *The Effect of Oil upon Waves.*—In reply to an objection of Admiral Bourgeois that the actual effect of oil upon waves should be fully tested before it is submitted to theoretical analysis, M. G. VANDER MENSBRUGGHE, replies that he has shown from incontestable facts that the wind produces upon the superficial layer of the sea, a horizontal motion of translation, which being sufficiently prolonged can communicate to the deeper layers, and can propagate to a great distance, very decided undulations. He has confined himself to a discussion of two cases; in the first, where the calm sea is covered with a thin layer of oil and is then submitted to the action of the wind; in the second, where the waves break. In the first case the formation of great waves is rendered impossible by the presence of the layer of oil. In the second, a simple calculation shows that the layer of oil exerts a great resistance at the base of the breaker, and thus compels it to extend itself and to subside very rapidly without producing severe wave shocks.—*Comptes Rendus*, Jan. 2, 1883, p. 62–63. J. T.

13. *Rarefied Air as a Conductor of Electricity.*—EDLUND continues his researches upon this subject. A number of experiments are described to show that the phenomena of the opposition to the passage of sparks from terminal to terminal in rarefied air cannot be explained by the theory that a vacuum does not conduct electricity. He carefully discusses the question of the contrary electro-motive force which is developed at the terminals. "It is not the resistance of the gas but this electro-motive force, increasing with the rarefaction and connected with the electrodes, that presents an obstacle to the passage of the current. Everything is in favor of the hypothesis that vacuum opposes a very feeble resistance to the propagation of electricity." Without the employment of electrodes, one can excite an induction current in a Geissler tube, which is sufficient to produce light. This would be impossible if the highly rarefied gas or vacuum were an insulator.—*Phil. Mag.*, Jan., 1883, p. 1–22. J. T.

14. *Conference for the Adoption of a Standard Meridian and of a Standard Time.*—The Minister of Public instruction has informed the French Academy that a circular has been received from the United States Government, which invites the French Republic to convoke a conference of all nations, which shall consider the question of the establishment of a common initial meridian and a common hour.

This circular states:—

(1.) That the need of uniformity is a great source of trouble in commerce—especially since the great extension of railroad systems and telegraph lines.

(2.) This question has been long discussed, both in Europe and America, by scientific men and by business men, who recognize the need of a general understanding and coöperation.

(3.) That the initiative seems to belong to the United States,

on account of the great extent of its territory in respect to longitude.

The President of the United States, convinced of the advantages of the reform in regard to the question, accordingly desires to obtain the opinion of the French Government in regard to an international conference. This communication will be referred by the French Academy to a commission from the Section of Astronomy and from that of Geography and Navigation.—*Comptes Rendus*, Jan. 2, 1883, p. 42. J. T.

15. *An Introduction to the Study of Organic Chemistry;* by ADOLPH PINNER, Ph.D., Professor of Chemistry in the University of Berlin. Translated and revised from the fifth German Edition by PETER T. AUSTEN, Ph.D. F.C.S., Professor of Analytical and Applied Chemistry in Rutgers College and the New Jersey State Scientific School. New York: John Wiley & Sons, 1883, pp. xix and 403. 12mo.—Dr. Pinner's Introduction is probably our best text-book on organic chemistry. After 14 pages of the most essential explanations, the mono-carbon group or methane compounds are described, including methane and its various halogen, hydroxyl, sulphur, nitrogen (cyanogen bodies) and other derivatives. The order chosen is that which most naturally leads from the simpler to the more complex bodies. The descriptions are full or brief according to the importance of the substance, and the genetic and constitutional relations of the compounds are brought out in a clear and concise manner. The methane derivatives occupy sixty pages. The ethane derivatives follow in similar order; followed again by their homologues up to the hexa-carbon bodies. The carbhydrates and uric acid derivatives are treated separately. Then follows a retrospect in which the more important paraffine, olefine and acetylene hydro-carbons, their halogen and hydroxyl derivatives (including aldehydes, ketones and acids of various basicity), amines, etc., etc., are tabulated so as to emphasize their homologies. These topics fill one-half of the book. Then follow benzene and its homologues and derivatives well presented in 100 pages. Naphthalene, anthracene and their congeners; the camphors, essential oils, resins, pyridine bases, alkaloids, glucosides, coloring matters, bitter principles, biliary substances and proteine bodies, complete the descriptive part. An Appendix of thirty pages gives an account of the methods of elementary analysis, the determination of gas-densities, the study of "constitution," action of re-agents and atomic migration. A complete Table of Contents and Index are supplied.

The book, we understand, closely follows Professor A. W. Hoffmann's plan of instruction; it is written in a very simple, clear and appropriate style. The translation as well as the revision does great credit to Professor Austen, and we cordially commend the volume to teachers and students of this fascinating and now most "practical" branch of science. S. W. J.

16. *Uniplanar Kinematics of Solids and Fluids*, with applications to the distribution and flow of electricity; by GEORGE M. MINCHIN, M.A. Oxford, 1882. 8vo, pp. 266.—Professor Minchin limits himself in this book to motion which takes place in one plane or parallel to one plane. In fluids, for example, no displacements are supposed perpendicular to the plane xy. The extension of uniplanar theorems and formulas to motion in three dimensions can be more easily made after the leading conceptions are mastered. The first half of the volume is occupied with the subjects of displacement, velocity, acceleration, epicycloidal motion, mass-kinematics, and small strains. In the treatment of epicycloidal motion, especially, the author has introduced new theorems.

The last half of the volume is devoted to kinematics of fluids and notes. It will be appreciated by persons who undertake to read Clerk-Maxwell's *Electrictity and Magnetism*, to which it is an admirable introduction. The style of Professor Minchin is very clear and concise. H. A. N.

II. GEOLOGY AND MINERALOGY.

1. *Transactions of the Wisconsin Academy of Sciences, Arts and Letters.* Vol. V, 1877–81. Madison, Wis.—Professor T. C. Chamberlin has here a paper entitled "Observations on the recent Glacial drift of the Alps, based on personal observations." In it, after describing the *Jardin* (or Garden) in the Chamouni region as an area of grasses and flowers surrounded on all sides by perpetual snow and ice, he compares with it the "driftless area" of Wisconsin. He observes that the Jardin is not a driftless area, for it was formerly under the ice-sheet and has erratics on its surface; but in the present glacial condition, the glacier divides and passes around it to unite again below, leaving it so far as present action is concerned a non-glaciated area, surrounded on all sides by active glaciation. It is stated that the resemblance ceases here; for it is enclosed by a steep moraine, thrust up by the ice around it, while on the border of the Wisconsin driftless area the glacial debris thins out gradually and disappears in an obscure margin. Further, the Jardin lies on the lee side of a prominence, and owes its escape from ice "more to its own prominence than to the effects of adjacent depressions." The ice moving from the Archæan region south of Lake Superior, after a course of 100 miles, "terminated on the declivity, and its waters continued on across the driftless area, leaving gravel terraces along their course."

On page 276, Professor J. E. Todd describes fossil foot-tracks on Potsdam sandstone from a locality four miles north of New Lisbon. The tracks are in involved lines or bands, where broadest 4 to 4½ inches wide, and have the characters of *Climatichnites* of Logan. Mr. Todd names them *C. Fosteri*, and speaks of them as undoubtedly of animal origin. An excellent figure accompanies the article.

2. *Lower Devonian fossil-bearing metamorphic rocks in the region of Bastogne, Belgium (town of Luxembourg).*—A paper by A. Renard, on the lithology of these metamorphic Devonian rocks, is published in Volume I (1882) of the Bulletin of the Belgian Royal Museum of Natural History. The beds belong to the middle member of the Coblentzian group of Dumont, called the Taunusian. The metamorphism and fossiliferous feature of the rocks were recognized by Dumont. The fossils *Spirifer macropterus*, *Chonetes sarcinulatus* and others, occur in a garnetiferous schist and quartzyte, besides remains of plants. The rocks include a hornblendic or actinolitic eurite, garnetiferous and chloritic quartzytes, garnetiferous fossil-bearing quartzyte, and chloritic garnetiferous schist also fossiliferous. M. Renard has studied the rocks both by chemical analysis and the microscope. He found in the hornblendic quartzyte, 30·62 of quartz, 37·62 of hornblende, 20·85 of mica, 4·14 of garnet, 1·02 of titanite, 1·51 of apatite, and 4·80 of graphite (visible in scales), with some ottrelite. The actinolitic rock consists of quartz 52·36, hornblende 46·73, with traces of titanic iron, mica and graphite. The hornblende (actinolite) is in interlaced fibers. The garnetiferous slate consists chiefly of biotite and garnet, with graphite in scales, and rarely hornblende. It graduates into the quartzyte. The author discusses also the origin of the metamorphism, and gives analyses of the ottrelite.

3. *On a large mass of Cretaceous Amber from Gloucester Co., New Jersey;* by Geo. F. Kunz. (Read before the New York Academy of Sciences, February 5th, 1883.)—About twelve months ago a mass of amber of uncommon size and shape (being twenty inches long, six inches wide and one inch thick, and weighing sixty-four ounces), was found at Kirby's marl pit, on Old Man's Creek, near Harrisonville, Gloucester Co., New Jersey. A one-quarter inch section showed a light grayish yellow color. A section one and one-quarter inch thick showed a light, very transparent yellowish brown color. The entire mass (surface and interior) was filled with botryoidal-shaped cavities filled with glauconite or green sand, and a trace of vivianite. The hardness is the same as the Baltic amber, only slightly tougher and cutting more like horn, and the cut surface showing a curious pearly luster, differing in this respect from any other amber I have yet examined. This luster is not produced by the impurities, for the clearest parts show it the best. It admitted of a very good polish. The specific gravity of a very pure piece of the carefully selected amber is 1·061, which is the lowest density on record, the usual amber range being from 1·065 to 1·081. It ignites in the same way as other ambers. It was found at a depth of twenty-eight feet, in and under twenty feet of the green sand or marl, the amber being found in a six-foot stratum of fossils, consisting mostly of *Gryphea vesicularis*, *Gryphea Pitcheri*, some *Terebratula Harlani* and others; the upper part of the marl, consisting of a large layer of limestone several feet in thickness, filled with

Palorthis, Echinoid spines and an occasional shark's tooth of the genus *Lamna*, and this covered with eight feet of earth. The marl belongs to the middle bed of the upper Cretaceous series.

No analysis has as yet been made of this amber, but the similarity in specific gravity, hardness and ignition leaves little doubt of its being true amber, or of its having been derived from a gum closely resembling that which is the source of the Baltic and other ambers.

III. Botany and Zoology.

1. *Apropos des Algues Fossiles, par le Marquis de Saporta.* Paris: Masson. Imp. 4to, pp. 82 + 10. 1882.—Nathorst, a distinguished Swedish naturalist, after making a series of observations and experiments upon the tracks and traces made by various invertebrate animals upon a beach or upon other soft and plastic material, published a paper (in 1881) in which he lays down the conclusions that no small part of the markings which have been described as fossil *Algæ* are simply the vestiges of such traces. And he pronounces this to be true of most of the fossil *Algæ* described in Saporta's *Paléontologie Française* and in his later and more popular volume, *L'Evolution du Règne Végétal.* The present volume is a courteous and magnificent reply to Nathorst's criticism—magnificent, for he devotes to it this beautiful imperial quarto volume, illustrated by figures interspersed in the text, as well as by ten lithographic plates,—courteous, for he over and over acknowledges the entire correctness of his adversary's facts and his conscientiousness in deducing from them such damaging conclusions. But he proceeds to rebut Nathorst's inferences as to most of the objects in question, by rehearsing the whole evidence in detail and presenting a series of figures, beginning with the most unequivocal instances, passing on to those that may be questionable, and allowing that the laudable desire of the phyto-paleontologists to include in their survey all the plant-like markings they know has led them to notice and describe not a few which are susceptible of the interpretation given by the Swedish naturalist. But to the greater part Saporta insists that the adverse conclusions are far-fetched, forced, founded on a preconception, and in certain cases capable of complete disproof. Also that some of the most dubious markings, which might well receive Nathorst's explanation, have been found quite closely imitated by the traces of an *Ulva*. A. G.

2. *Les Plantes Potagères, Description et Cultures des Principaux Légumes des Climats Tempérés;* par Vilmorin, Andrieux & Cie. 1883, pp. 650, 8vo. Paris.—Besides its importance to cultivators, this volume—prepared by a most competent and trusty hand, and issued by the noted house of Vilmorin, Andrieux & Co.—has no small botanical value. It treats of the kitchen-garden plants of temperate climates, with considerable fulness and abundant illustration from original sketches. It refers the varieties and races to their proper botanical species, the native country of which is

indicated with all the correctness to which recent researches have attained. So that this book, as far as it goes, is a good companion and supplement to the contemporary *Origine des Plantes Cultivées*, by DeCandolle, a notice of which is still due to our readers. Among the interesting matters contained in the Introduction, we note the statement of the author that cultivation, even where immemorial, has in no wise effaced the limit of species. A. G.

3. *The Colors of Flowers;* by GRANT ALLEN. Macmillan & Co. 1882.—This taking little book, in which a popular subject is very interestingly treated, originated as an article in the Cornhill Magazine, was extended into a series of articles in *Nature*, and now the latter are reëdited and collected in a volume of *Nature Series*. We need only notice the distinguishing feature of a contribution to evolutionary science which must already have been widely read. The "central idea" is that petals are transformed stamens, rather than transformed leaves. The argument is, that the earliest flowers consisted only of stamens and pistils, one or both; that the original color of these was yellow, that consequently (by inheritance) the stamens of almost all flowers are yellow,—whence "it would seem naturally to follow that the earliest petals would be yellow too." Now "the earliest and simplest types of existing flowers [i. e. the petals of such as have any] are almost always yellow, ... and this in itself would be a sufficient ground for believing that yellow was the original color of all petals." The author thence proceeds to show how this color is changed into others, white, red, &c., up to blue, in a normal succession.

The experienced botanist, looking at the facts irrespective of theory, will raise some questions. Is it actually true that most of the simpler flowers with colored perianth are yellow, and that this color is conspicuously absent from highly differentiated and attractive flowers? No numerical proof of it is offered, and we suppose no convincing proof is to be had from observation. Yellow vastly preponderates in the very largest of the highly differentiated orders of plants, and holds a fair share in the two next largest (*Leguminosæ* and *Orchideæ*). Exception may also be made to the fundamental premise that the stamens of almost all flowers are yellow. The pollen is prevailingly of this color, and so more commonly may be the anther, but rarely the filament, the dilatation of which is assumed to give rise to petals:—to give rise, moreover, to the sepals also, if the theory holds, at least when they are colored. But how when they are green and herbaceous? And how is the line to be drawn; and if colored sepals originated from stamens, why not subtending bracts as well, when these are petaloid?

Our author says: "We can see how petals might easily have taken their origin from stamens, while it is difficult to understand how they could have taken their origin from ordinary leaves, a process of which, if it ever took place, no hint now remains to us." But either we have a hint in the brilliant bracteal

leaves of Painted-cup, Poinsettia, and *Salvia splendens*, or these attractive leaves have also taken their origin from stamens! Whether we choose to regard petals and other perianth-members as modifications of stamens or as modifications of green leaves is, according to our thinking, mainly a question of mode of conception. Some good morphological evidence may be adduced for either. Mr. Allen's study of the case by evolutionary deduction is interesting and suggestive. It has the advantage of making an appeal to facts open to observation. We are by no means convinced that the facts sustain it. A. G.

4. *Direct observation of the movement of Water in Plants.* M. JULIEN VESQUE (Ann. des Sc. Nat., XV, 1) has devised a simple method of demonstrating the transfer of water in the stems of plants, which promises to have a wide application. The stem is cut obliquely during immersion in water, and the thin part of the severed stem is placed in the field of the microscope, of course completely wet on the cut surface. After the cover-glass is adjusted and the stem is securely fastened, so that it cannot be easily disturbed by subsequent treatment, a very little freshly precipitated calcium oxalate, or other finely divided substance, is introduced under the cover. If the leaves have not been removed from the stem, a rapid current is at once observed to flow towards the cut surface. The insoluble salt collects at the open mouths of the vessels, often passing into the capillary tubes after a temporary arrest, and the same phenomenon is repeated several times as the minute plugs are formed and then sucked in.

With low powers of the microscope it is possible to use a second slip instead of the thin cover, and then the simple apparatus can be held more firmly in its place. In any case it is possible to measure the rapidity of the current by means of a micrometric eye-piece; and several such rates are given.

When the stem is quickly stripped of its leaves the current is stopped at once. But when, on the other hand, a leaf or a part of the stem is pinched, there is immediately a backward flow of water.

It is well known that two conflicting views have been held by physiologists as to the channel by which the upward movement of water in wood takes place. Some think that the transfer is solely by imbibition, and that no free water is carried from cavity to cavity of the wood-element, or rather, that no free water exists in the cavities. Others have held that free water is carried from one wood-element to another, and that the walls themselves play only a subordinate role. To these opposed views may be added a third, which appears to be a compromise; namely, that water in a free state actually exists as a thin lining on the cell-wall. The chief advocate of the latter view has however abandoned it in favor of the imbibition theory. A recent publication by Elfving (Bot. Zeit. Oct. 1882) details the results of experiments which considerably strengthen the "cavity" theory. Now just at this point come observations of Vesque, in a continuation of the paper regarding the method of direct demonstration, which

go far towards showing that here, as was long ago suspected, the truth is to be found between the extremes. These experiments, which need to be carefully repeated, indicate that under certain circumstances the transfer of water takes place by means of the cavities themselves, but that in all cases they may serve the part of reservoirs.

Moreover, the caliber and length of the vessels regulate the rate of transpiration; resistance to the movement of the water following the law of Poiseuille, so that the resistance is inversely proportional to the fourth power of the diameter and directly proportional to their length. We give in full the close of Vesque's paper:

"It is evident that p having reached its maximum, that is to say the suction resulting from transpiration not being able to increase without changing our conditions, because the air dissolved in the water becomes disengaged, the quantity of water which arrives at the organs of transpiration across a vessel filled with water is expressed by $\frac{Ad^4}{l}$. From this we can see why climbing plants have such large vessels; in fact, the increase in diameter can alone compensate that of the length. And, further, the quantity of water which can pass through a vessel in a given time bears a certain relation, varying for each species, with the water which it contains: this, which I have called the transpiratory reserve, it might be better to term the vascular transpiratory reserve. I propose to publish a work on water reservoirs in general. A study of this apparatus very often gives the key as to the resistance of certain plants to certain surroundings, and permits us to indicate at once the conditions under which we must cultivate plants. Anatomy, I am convinced, will open the way to rational culture." G. L. G.

5. *The stalked Criniods of the Caribbean Sea;* by P. Herbert Carpenter, Bulletin of the Museum of Comparative Zoology, vol. x, No. 4, Dec., 1882.—This paper is a Report on the Dredgings of the Coast Survey Steamer Blake, in the Gulf of Mexico and Caribbean Sea, in 1877–1879, under the supervision of A. Agassiz. This report contains descriptions of four species of *Pentacrinus* (out of the eight known), and two of *Rhizocrinus: P. asteria, P. Mülleri, P. decorus* and *P. Blakei* (here new), *R. Lofotensis* and *R. Rawsoni.* They were obtained at depths between 73 and 955 fathoms, but mostly between 80 and 400. Mr. Carpenter states that stalked crinoids have been obtained at depths exceeding 650 fathoms only in fourteen instances, the lowest limit being 2,435 fathoms for *Bathycrinus gracilis,* found by the Porcupine in 1869; that *P. Wyville-Thomsoni* was obtained by the Porcupine in 1,095 fathoms in 1870: *P. Naresianus,* at 1,350 fathoms, by the Challenger in the Pacific; that *Bathycrinus* ranges from 1,050 to 2,435 fathoms, *Hyocrinus* from 1,600 to 2,325; while *Rhizocrinus Lofotensis* occurs in the Norwegian fiords at 80 fathoms, and in 175 to 955 in the Caribbean Sea.

Mr. Carpenter adds "it is a pity that we have no later knowledge of the "Australian Encrinite," a stem six inches long, which was obtained by Poore (Ann. Mag. Nat. Hist., ix, 486, 1862), at a depth of 8 fathoms in King George's Sound."

6. *Selections from Embryological Monographs*, compiled by A. AGASSIZ, WALTER FAXON and E. L. MARK. I. *Crustacea*, by W. FAXON, with 14 plates. Memoirs of the Museum of Comp. Zool., vol. ix, No. 1, 4to.—This volume is the beginning of a series of publications which will constitute a most generous contribution to American science by the Museum of Comparative Zoology. The fourteen crowded but admirably engraved plates contain full illustrations of the embryological development of various species of Crustacea, Limulids and Pycnogonids, derived from recently published Memoirs. The authors cited from include, besides Mr. Faxon and Professor A. Agassiz, Fritz Müller, W. Lilljeborg, Carl Claus, E. and P. J. Van Beneden, E. Melschnikoff, N. Bobretzky, F. Richters, Paul Mayer, B. Ulianin, L. St. George, H. Reichenbach, H. Rathke, Anton Dohrn, A. v. Nordmann, C. Grobben, P. P. C. Hoek, C. Darwin, T. H. Huxley, C. Spence Bate, J. Barrande, G. Hodge, W. K. Brooks and A. S. Packard, Jr.

Transactions of the Linnæan Society of New York. Vol. I. 168 pp. large 8vo. Published by the Society, Dec., 1882. New York.—This handsomely printed volume contains the following papers: The Vertebrates of the Adirondack region, Northeastern New York (Carnivora), by C. H. MERRIAM, M.D.; Is not the Fish-Crow (*Cervus ossifragus* Wilson) a winter as well as a summer resident at the northern limit of its range? by WM. DUTCHER; A review of the summer birds of a part of the Catskill Mountains, with prefatory remarks on the Faunal and Floral features of the region, by E. P. BICKNELL.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Cold of January in Iowa.* From the Iowa Weather Bulletin for January, 1883; by GUSTAVUS HINRICHS.—The great cold spell from the 10th to the 23d has been of so extraordinary a character that possibly the following table, giving the temperature of the air as registered by the standard thermometer at the Central Station may be of interest. The minus sign — indicates degrees below zero, Fahrenheit.

January.	Friday 19.	Sat. 20.	Sun. 21.	Mon. 22.	Tues. 23.	Wed. 24.
2 A. M.	21	−12	−24	−20	−18	0
4 A. M.	21	−11	−25	−21	−18	1
6 A. M.	5	−12	−22	−22	−18	0
8 A. M.	− 8	−12	−18	−22	−19	0
10 A. M.	− 9	−13	−13	−15	−17	5
Noon	− 9	−13	−10	−10	−11	12
2 P. M.	− 8	−12	− 9	− 8	− 6	16
4 P. M.	− 8	−12	−10	− 7	− 6	15
6 P. M.	−10	−14	−14	−10	− 6	14
8 P. M.	−12	−18	−16	−13	− 4	10
10 P. M.	−13	−20	−18	−14	− 2	9
Midnight	−13	−23	−19	−15	0	5

2. *Report on the Climatic and Agricultural Features and the Agricultural practice and needs of the Arid regions of the Pacific slope, with notes on Arizona and New Mexico.* Made under the direction of the Commissioner of Agriculture, by E. W. HILGARD, T. C. JONES and R. W. FURNAS. 182 pp. 8vo. Washington, 1882.—This report is full of valuable facts and discussions respecting the agricultural and other field-resources of the Pacific slope, and particularly of California. The special subjects are: climates, soils, timber and forest culture, causes of aridity, irrigation, alkali lands and methods of improvement, analyses of soils and waters, California wines and brandies, with notes on exotic fruits and useful plants on trial at the Agricultural College of the University of California, and on the Phylloxera in California, by E. W. Hilgard; on field crops and animal industries, by T. C. Jones; on the standard sugar refinery, Alvarado, California, raisin making, olive industry, etc., by R. W. Furnas.

3. *Astronomical and Meteorological Observations made during the year* 1878, *at the U. S. Naval Observatory.*—The two appendices in this volume, Professor Holden's Monograph on the Nebula in Orion, and the Longitude of the Observatory at Princeton, N. J., have already been noticed in this Journal. The volume contains, besides these, the regular observations made at the Naval Observatory with the large and small equatorials and the transit circle.

4. SCIENCE.—The first number of Science, the prospectus of which was noticed on page 87, appeared on the 9th of February.

Report of an examination of the Upper Columbia River and the territory in its vicinity in September and October, 1881, to determine its navigability, and adaptability to steamboat navigation, made by direction of the Department of the Columbia, by Lieut. T. W. SYMONS, Corps of Engineers, U. S. A. 134 pp. roy. 8vo, with maps. Washington. 1882.

Bulletin of the U. S. Fish Commission, Spencer F. Baird, Commissioner, vol. I, 1881, 466 pp. 8vo. Washington. 1882.

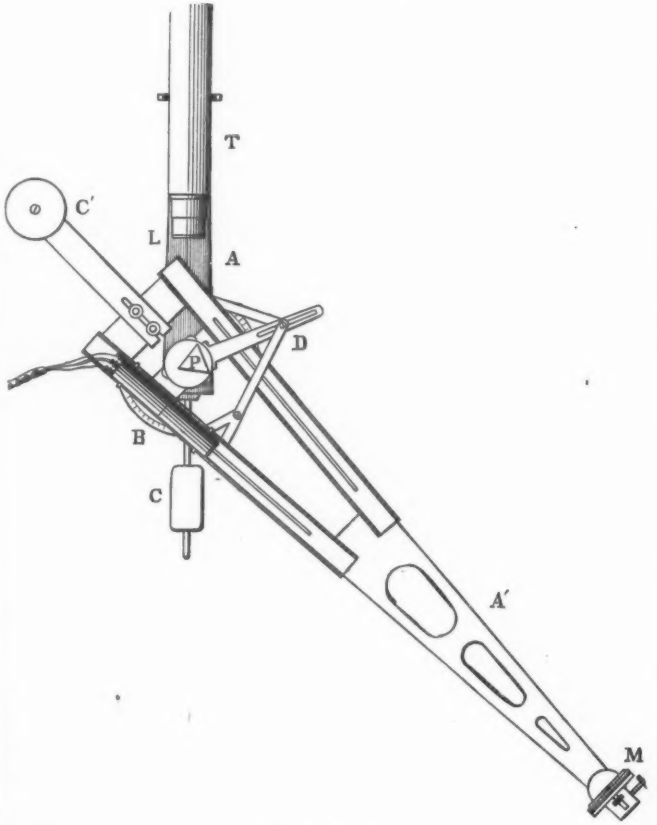
The American Palæozoic Fossils, of S. A. MILLER, Cincinnati. 334 pp., large 8vo. This second edition of Mr. Miller's useful work, mentioned on page 474 of the last volume of this Journal as in the press, has been published. The additions cover 90 pages.

Report of the Entomologist to the Department of Agriculture for the year 1881, by CHARLES V. RILEY, M.A., Ph.D. 214 pp. 8vo, with 19 plates, partly colored.

Die elektrische Kraftübertragung und ihre Anwendung in der Praxis. Mit besonderer Rücksicht auf die Fortleitung und Vertheilung des elektrischen Stromes: dargestellt von Eduard Japing, dipl. Ingenieur. 236 pp. 12mo, with 45 cuts. Wien and Leipzig (A. Hartleben) Elektrotechnische Bibliothek, Band II.

OBITUARY.

ALEXIS PERREY, eminent for his long-continued labors in connection with the department of Earthquakes, and Honorary Professor of the Faculty of Sciences at Dijon, died at Paris, on the 29th of December last, in his 76th year.



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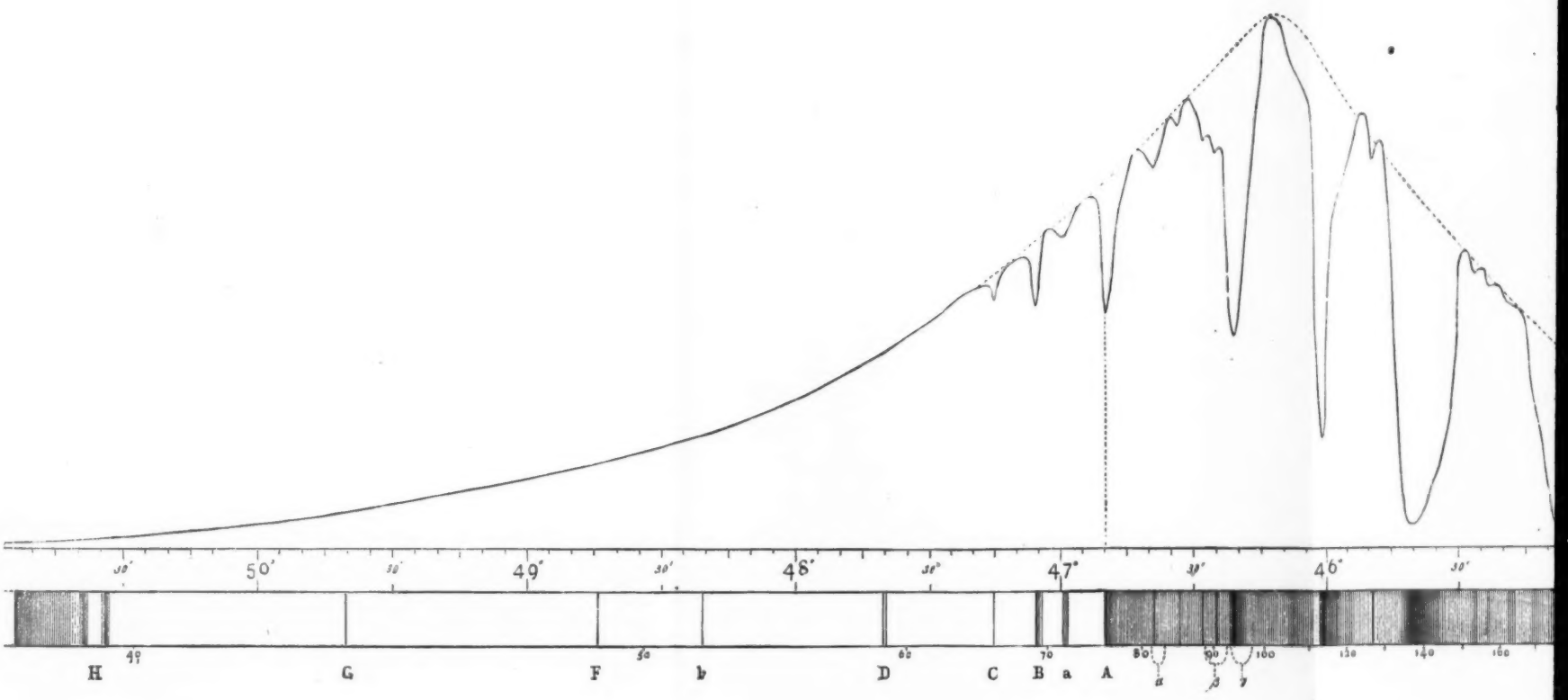
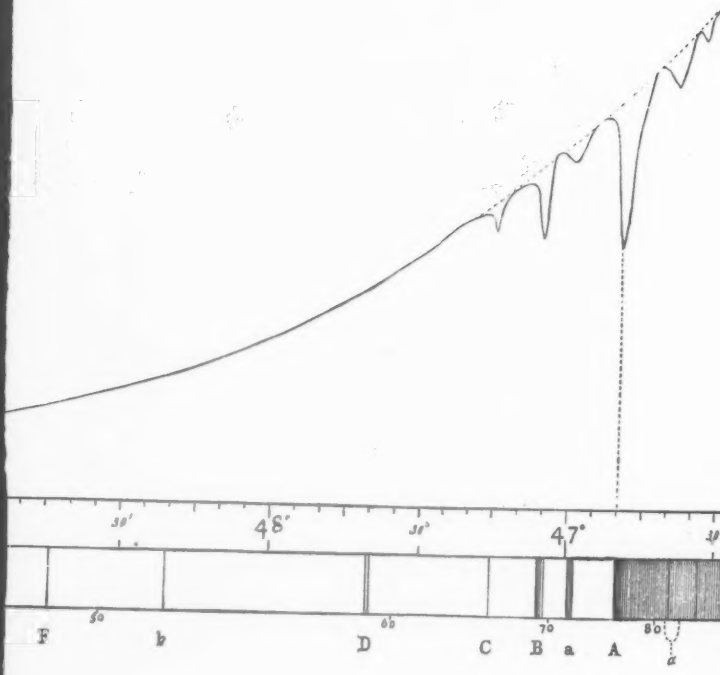


PRISMATIC SPECTRUM.

AM. JOUR. SCI., Vol. XXV, 1883 Plate III



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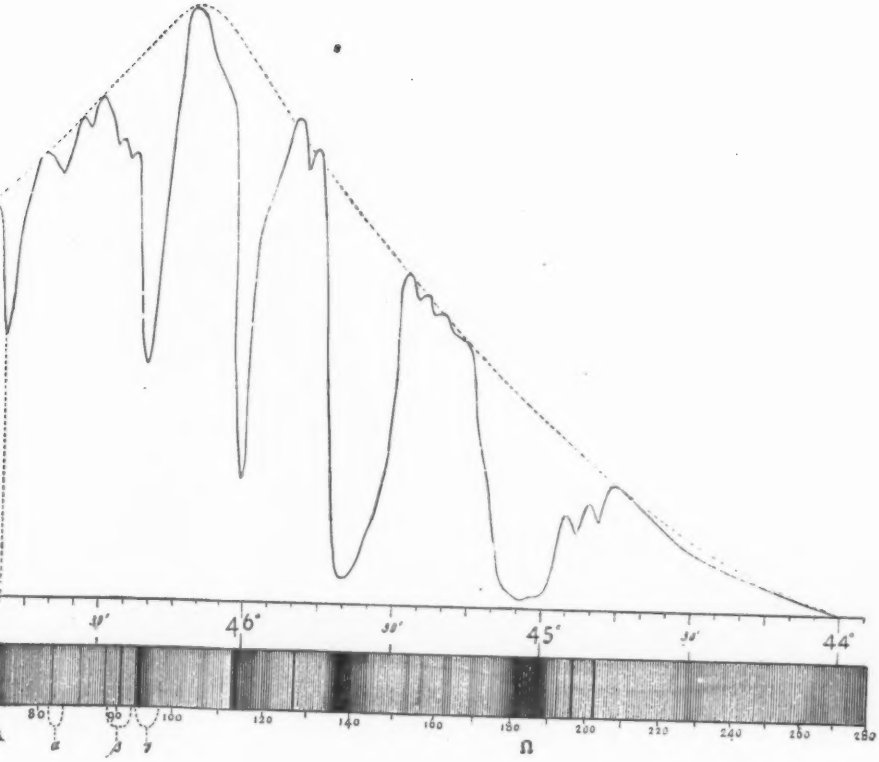
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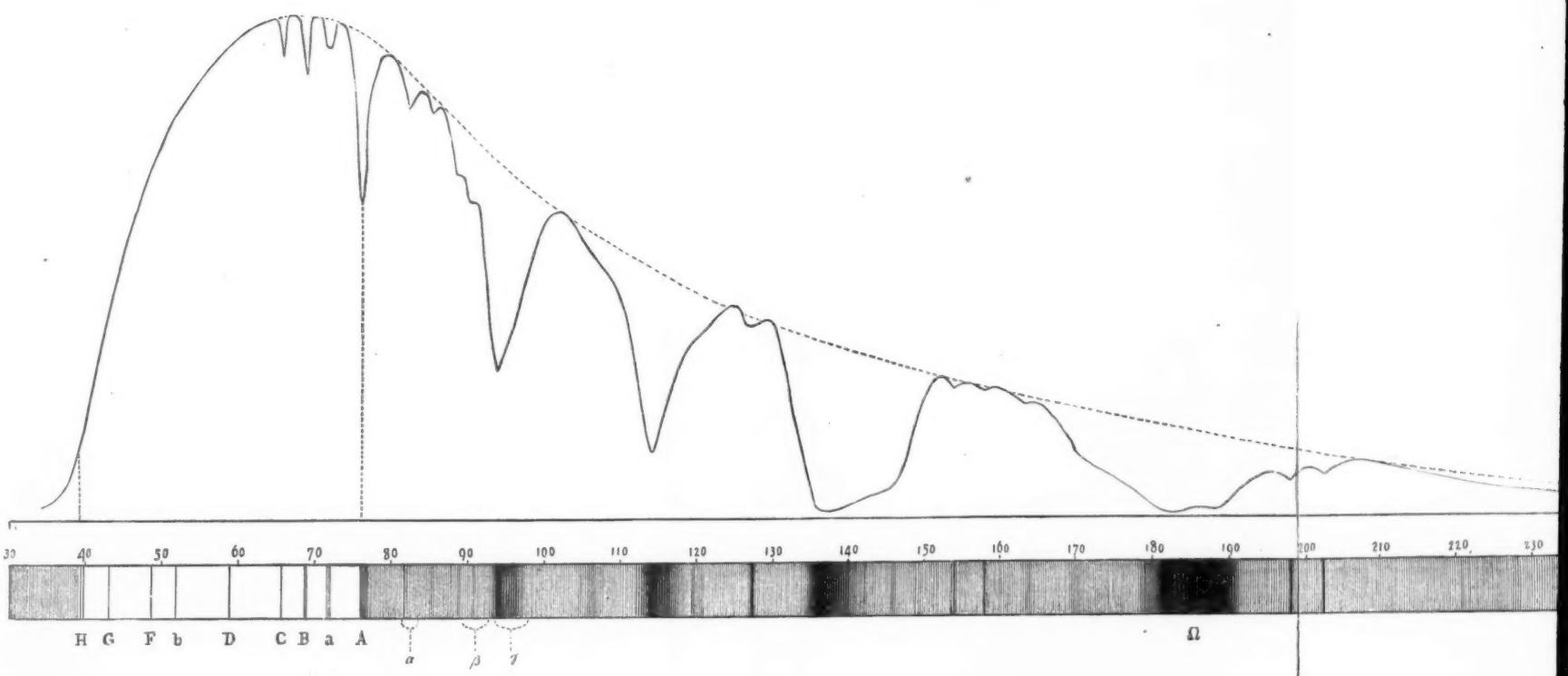
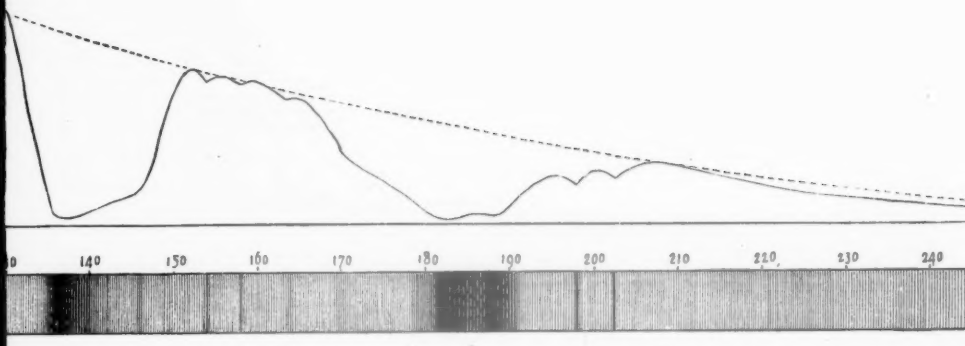


NORMAL SPECTRUM.

AM. JOUR. SCI., Vol. XXV, 1883 Plate IV.



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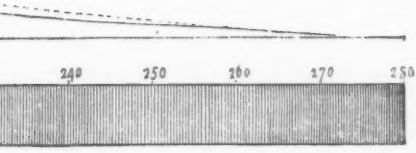
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